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Major P. A. MACMAHON, D.Sc., Sc.D., LL.D., F.R.S., PRESIDENT, in the Chair.

Arthur Brinley Mayne, B.A. (Oxon.), Charterhouse School, Godalming, Surrey; and 3 Bryn-y-mor Crescent, Swansea, South Wales;

Wilfred C. Parkinson, Department of Terrestrial Magnetism of the Carnegie Institution of Washington, The Observatory, Perth, Western Australia;

Thomas James Richmond, B.Sc. (Lond.), Hydrographic Department, Admiralty; and "Eastdene," St. James' Road, Sutton, Surrey; and

Rohani Thirunal Raja M. Rama Varma Raja, Prince of Mavelikara Palace, Assistant Director and Astronomer, H.H. the Maharaja's Observatory, Trivandrum, Travancore, S. India,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended:—

Philip Francis Everitt, B.Sc., Optical Instrument Designer, St. Heliers, Oxgate Gardens, Cricklewood, N.W. 2 (proposed by H. H. Turner);

E. H. S. Flood, Canadian Trade Commissioner, Barbados, West Indies (proposed by Capt. L. A. Demers); and

Lieut. Hugh Duncan Grant, R.N.V.R., L.A., F.R.Met.Soc., N1 Section, Naval Air Department, Air Board Office, Admiralty (proposed by Hugh Walsham).

Forty-six presents were announced as having been received since the last meeting, and the thanks of the Society were returned to the respective donors.

On the Origin of Ptolemy's Catalogue of Stars.

By J. L. E. Dreyer, Ph.D.

For more than a hundred years it has been the prevailing opinion among astronomers that Ptolemy had borrowed all his star-places from the catalogue of Hipparchus. This was supposed to be proved by the fact that Ptolemy's longitudes are on an average fully a degree too small, while he assumed the constant of precession to be 36″ instead of 50″. The epoch of Ptolemy's catalogue is A.D. 137, that of Hipparchus 129 B.C.; the interval is therefore 265 years, and $265 \times 14'' = 1^\circ\ 2'$, *q.e.d.*

But the question is not, after all, so easily decided. It is interesting to see that the late C. H. F. Peters, to whose labours, jointly with those of Mr. Knobel, we owe the recently published invaluable edition of the catalogue, three years before his death pointed out that the constant error of the longitudes might very well have arisen in some other way.* Deferring for the present the consideration of this question, we shall first consider some information concerning the catalogue of Hipparchus which has come to light since Peters' death, and which strongly militates against the prevailing view.

That Hipparchus was a careful observer who must have produced a number of star-places as accurate as the imperfect instruments then in use would allow, is shown by his list of 44 time-stars, which follow each other at intervals of exactly one hour. Schjellerup has shown that these stars had been so well chosen that they gave the time every hour within a minute.† We also know that Hipparchus possessed a better knowledge of the true value of the constant of precession than Ptolemy did; for though he first announced that the equinoxes moved at least 36″ per annum, he afterwards stated that he had found the longitude of Spica to have increased 2° in the 154 years elapsed since the time of Timocharis, which gives 46″·8.‡ It is therefore much to be regretted that his star-catalogue has not been preserved. If Ptolemy had merely added a certain constant quantity to the longitudes of Hipparchus to bring them up to his own epoch, it would be reasonable for us to assume that the 1025 stars in Ptolemy's catalogue must all have occurred in the catalogue of

* Short report of a speech made by Peters at the Kiel meeting of the Astronomische Gesellschaft in 1887, *Vierteljahrsschrift*, xxii. p. 269.

† "Sur le chronomètre céleste d'Hipparque," in the journal *Copernicus*, vol. i. p. 25 (Dublin, 1881). This valuable paper appears to be quite unknown to Dr. Manitius, editor of *Hipparchi in Arati et Eudoxi Phœnomena Commentarii* (Lipsiæ, 1894).

‡ *Syntaxis*, vii. 2, ed. Heiberg, ii. p. 13.

Hipparchus. It is therefore of importance for us to know how many stars the older catalogue contained.

The only information on this point which we possessed until lately was given by an anonymous commentator to Aratus.* According to him, Hipparchus said that there were 1080 stars inside the constellations; that is, not counting the "informes" or stars not actually inside the figures of the constellations. Subtracting the informes, Ptolemy has only 918 stars inside the constellations. But, as remarked by Boll in the paper presently to be mentioned, it is incredible that Ptolemy should have omitted 162 or nearly one-sixth of the whole number. Either the passage is corrupt, or it is simply an estimate of the number of more or less conspicuous stars, like that given by Pliny when he says that some people have counted 1600 stars.†

But in 1892 there was first brought to light a hitherto unknown Greek list of the constellations, attributed to Hipparchus; and since then several copies of it have been found, the best one being part of Cod. Parisinus 2506, an astrological MS. of the fourteenth century. The title is simply *ἐκ τῶν ὑπάρχων ἀστέρων*. It has been discussed by F. Boll in a paper published in 1901,‡ in which the whole of the short text is given; it simply counts up the constellations and adds to each a number, being the number of stars in it, as follows:—

Ursa major	24	Triangulum	3	Canis major	21
Ursa minor	7	Perseus	19	Canis minor	3
Draco	15	Auriga	8	Cancer	16
Bootes	19	Hydra	27	Leo	19
Corona borealis	9	Crater	10	Virgo	19
Hercules	24	Corvus	7	Libra	4
Ophiuchus	17	Argo	13	Scorpio	15
Lyra	10	Centaurus	26	Sagittarius	16
Cygnus	14	Lupus	13	Capricornus	26
Aquila	4	Ara	4	Aquarius	18
Sagitta	4	Corona australis	...	Pisces	41
Delphinus	9	Piscis australis	12	Aries	17
Pegasus	18	Cetus	14	Taurus	18
Cepheus	19	Eridanus	...	Gemini	19
Cassiopea	14	Orion	18		
Andromeda	20	Lepus	...		

* *Petavii Uranologion*, p. 262, second ed. in *Petavii Opus de doctrina temporum*, t. iii. (Antwerp, 1703), p. 145. Maas, *Commentariorum in Aratum reliquiæ*, Berlin, 1898, p. 128.

† In this passage (*Hist. Nat.*, ii., § 110) it is also said that there are 72 constellations. Ptolemy has only 48. If we add to these Coma and Antinous (which he omitted) and his 22 groups of informes, we get 72. Can that perhaps be the explanation?

‡ F. Boll, "Die Sternkataloge des Hipparch und des Ptolemaios," in the *Bibliotheca mathematica*, 3rd series, vol. ii. pp. 185–195.

In all 46 constellations. Of Ptolemy's only Equuleus and Serpens are missing, the former possibly because it was not used by Hipparchus, the latter certainly only owing to a slip on the part of the original compiler. In 43 cases the number of stars is given. In 30 cases the numbers are exactly the same as in the Katasterismoi of Pseudo-Eratosthenes, now known to be a compilation made in the third (or the end of the second) century A.D. Of the other 12 cases (neglecting Argo, the number belonging to which must be very erroneous), only one, Cancer, has a smaller number than the Katasterismoi. In the 11 remaining constellations (Bootes, Hercules, Lyra, Centaurus, Lupus, Cetus, Orion, Canis major, Sagittarius, Capricornus, Aquarius) Hipparchus has from one to five stars more than Pseudo-Eratosthenes.

According to Boll, everything seems to confirm the idea that this list really gives the number of stars observed by Hipparchus. In his earlier work, the Commentary to Aratus, Hipparchus gives more stars to some constellations than this list does. But it should be remembered that there is no finality about lists of that kind. In 11 constellations Ptolemy has fewer stars than the Katasterismoi and the new list, viz. Corona, Cepheus, Cassiopea, Hydra, Crater, Canis major, Canis minor, Aries, Gemini, Cancer, Pisces. But this is probably solely caused by drawing the constellations in a different manner.* To draw the figures of the constellations so as to make them resemble human figures or other objects, it was necessary to put some stars among the ἀμόρφωτοι (informes), as already done by Aratus; and this was probably also done by Hipparchus, so that he in his great star-catalogue gave some constellations narrower limits than he had done in his earlier work, the Commentary to Aratus.

If we omit Argo, we have then 42 constellation figures containing 640 stars. In the same (Aquarius without the water and Ophiuchus without the serpent) Ptolemy has 772 stars.† In the remaining seven figures (Serpens, Aqua, Equuleus, Eridanus, Lepus, Argo, Corona australis) Ptolemy has 146 stars. Assuming in these the same proportion between the two catalogues, it would appear that Hipparchus must have had 121 stars, so that his catalogue must have contained $640 + 121 = 761$ stars. But this estimate does not include the informes, though there are some important stars among them, *e.g.* Arcturus. It is possible that Hipparchus (or the compiler of the list) included these in the nearest constellation, as Ptolemy did. If not, we must add some informes to the 761, and as Ptolemy has in all 108, Hipparchus may have had about 90. The star-catalogue of Hipparchus therefore contained at least 851 stars, and at most 20 or possibly 30 more. For it is most improbable that, in the seven constellations for which we have no

* Compare *Syntaxis*, vii. 4, at end (Heib., ii. p. 37).

† 774, but two of these are duplicates, Herculis 29 = Bootis 9 and Tauri 21 = Aurigæ 11.

numbers, the proportion should be very different from that of the 42 constellations.

Assuming, then, that it has been proved by Boll in his paper (with which I have only lately become acquainted) that the catalogue of Hipparchus did not contain more than about 850 stars, I shall now try to show that *if* Ptolemy made use of these 850 star-places we are probably able to pick out most of the remaining 175 stars, which he must either have observed himself or taken from some other source.

In Ptolemy's catalogue the longitudes and latitudes are given to fractions of a degree, the fractions being generally multiples of $\frac{1}{6}$, as if the instrument used had been divided to 10′. But in a certain number of cases the fractions $\frac{1}{4}$ and $\frac{3}{4}$ of a degree occur, as if a different instrument had been used.* 142 stars distributed fairly well through the constellations have latitudes of which the fractions are either $\frac{1}{4}$ or $\frac{3}{4}$, while only three stars, all in Virgo, have longitudes distinguished in this way.† On reading Boll's paper it struck me at once that, as about 175 stars cannot in any case have been taken from the catalogue of Hipparchus, it was not unlikely that they were represented by the 145 stars plus some others the minutes of which are 30 or 60, and may therefore have been observed either with an instrument divided to $\frac{1}{6}°$ or with one divided to $\frac{1}{4}°$. The following table gives for each of the 42 constellations the number of stars in Ptolemy's catalogue, the number of stars of $\frac{1}{4}°$ or $\frac{3}{4}°$, the difference between the two sets, and the number of stars observed by Hipparchus.

Constellation.	Stars in Ptolemy's Cat.	Stars of $\frac{1}{4}$ and $\frac{3}{4}$.	Diff. 2-3.	Hipparchus.
Ursa minor . . .	7	0	7	7
Ursa major . . .	27	3	24	24
Draco	31	5	26	15
Cepheus	11	3	8	19
Bootes	21	2	19	19
Corona borealis . .	8	1	7	9
Hercules . . .	29	5	24	24
Lyra	10	1	9	10
Cygnus	17	1	16	14
Cassiopea . . .	13	2	11	14
Perseus	26	6	20	19
Auriga	13	1	12	8
Ophiuchus . . .	24	6	18	17
Sagitta	5	0	5	4
Aquila	9	0	9	4

* Peters in his speech in 1887 pointed out that two instruments must have been used.

† One of the three stars is among the informes Virginis.

Constellation.	Stars in Ptolemy's Cat.	Stars of $\frac{1}{4}$ and $\frac{3}{4}$.	Diff. 2–3.	Hipparchus.
Delphinus . . .	10	2	8	9
Pegasus	20	1	19	18
Andromeda . . .	23	0	23	20
Triangulum . . .	4	0	4	3
Aries	13	1	12	17
Taurus	33	6	27	18
Gemini	18	2	16	19
Cancer	9	1	8	16
Leo	27	4	23	19
Virgo	26	3	23	19
Libra	8	2	6	4
Scorpius . . .	21	1	20	15
Sagittarius . . .	31	2	29	16
Capricornus . . .	28	4	24	26
Aquarius . . .	22	5	17	18
Pisces	34	7	27	41
Cetus	22	1	21	14
Orion	38	8	30	18
Canis major . . .	18	4	14	21
Canis minor . . .	2	0	2	3
Hydra	25	4	21	27
Crater	7	0	7	10
Corvus	7	1	6	7
Centaurus . . .	37	6	31	26
Lupus	19	1	18	13
Ara	7	1	6	4
Piscis australis . .	12	3	9	12
Total . .	772	106	666	640

Some of the figures in the third column are of course too small, because, as already mentioned, we are unable to disentangle some of the additional stars from those observed by Hipparchus, $\frac{1}{2}°$ being both $= \frac{2}{4}°$ and $\frac{3}{6}°$ and $1° = \frac{4}{4}°$ and $\frac{6}{6}°$.* Otherwise the agreement between the last two columns would probably be still better. Two-thirds of the additional 145 stars are of magnitudes 4 to 6 (fully $\frac{1}{3}$ being of the fourth magnitude), and there is only one star of the second magnitude, Pollux. It is needless to say that a star like Pollux could not have been passed over by Hipparchus, but

* Of the 106 additional stars, 63 have latitudes with the fraction $\frac{1}{4}$ and 41 with $\frac{3}{4}$, while there are two stars of which the longitude has $\frac{1}{4}°$.

it may have been used by his successor as a connecting link between the earlier catalogue and the stars observed to fill gaps here and there. It is certainly strange that, with the exception of the three stars in Virgo, it is only latitudes which show signs of having been observed with a different instrument. The differences of longitude and of latitude of the new stars from a small number of standard stars must have been observed with the instrument described by Ptolemy, called astrolabon,* and the latitude circle must have been graduated to $\frac{1}{4}^\circ$ only.

The next question is: By whom were the additional stars observed, by Ptolemy or by some astronomer living in the interval between him and Hipparchus?

Of astronomical activity during that period of more than 250 years we know extremely little. Ptolemy only quotes a very few observations made by Menelaus and Agrippa during the last decade of the first century; but some details about the planetary orbits given by Pliny (especially the positions of the apsides of the excentrics) show that serious work in astronomy must have been done during that long interval. Some idea of this kind, combined with the error of Ptolemy's longitudes, must have suggested to Al Sufi the following remarkable statement† :—"As to the positions of the stars with regard to the signs of the Zodiac, I have found that Ptolemy built on the observations of Menelaus made in the year 845 of Nabonassar,‡ and that he has given in his book the positions of the fixed stars for the first year of Antoninus, that is, the year 886 of Nabonassar. The interval between the observation of Menelaus and the epoch of Ptolemy is, then, 41 years. Ptolemy says that Menelaus found the distance of Spica from the beginning of Cancer to be 86° 15′, and the distance of the most northerly of the three in the brow of the Scorpion from the autumnal equinox 35° 55′; whence it follows that Spica ought then to be in 26° 15′ of Virgo, and the most northerly of three in the brow of the Scorpion§ in 5° 55′ of Scorpius. Ptolemy believed the motion to be of 1° in 100 years. Adding then to each of the two stars 25′, which results from the motion in the interval of time between him and Menelaus, that is, 41 years, he fixed Spica in 26° 40′ of Virgo and the north star in the brow of the Scorpion in 6° 20′ of Scorpius; afterwards he added this same quantity to all the stars, and it is thus that he has prepared the tables which are in his book *Al-madjisti*."

There are several flaws in this very positive assertion. First, the epoch of Ptolemy is not 886 of Nabonassar, but 885,‖ so that the interval between him and Menelaus was 40 years, and the amount of precession (at the rate of 36″ per annum) 24′ 0″, not

* By Tycho Brahe called Armillæ Zodiacales.

† *Description des étoiles fixes*, ed. by Schjellerup, p. 42.

‡ Began in July, A.D. 97.

§ β Scorpii.

‖ This slip had already been made by Al Battani, *Opus astron.*, cap. 51, ed. Nallino, p. 124.

25′, a mistake Ptolemy is not very likely to have made. Secondly, Ptolemy does not say that Menelaus found the longitudes of Spica and β Scorpii to be 170° 15′ and 215° 55′. He merely quotes two conjunctions of the moon with these stars observed by Menelaus in January A.D. 98, and from these he himself deduces from his own lunar tables the positions of the moon and consequently of the two stars for that year. It is, however, quite true that these positions differ from those in Ptolemy's catalogue by 25′. Ptolemy uses them for determining the amount of precession by comparison with the observations of Timocharis in the year 454 Nabonassar. Otherwise he never mentions Menelaus. What is then Al Sufi's authority for the assertion that *all* Ptolemy's star-places are due to Menelaus, 25′ having been added to the longitudes? There is no sign anywhere in Al Sufi's book that he had access to any Greek astronomical observations, except those recorded in the *Syntaxis*. He accepted Al Battani's estimate of annual precession, 1° in 66 years or 54″·5 per annum; he knew that Al Battani, the greatest Arabian astronomer up to his own time, had (like Ptolemy) made use of some observations made by Menelaus to fix that constant, and he must have jumped to the conclusion that as Ptolemy's erroneous precession was due to the use he had made of observations by Menelaus and Agrippa 40 years before his own time, the whole of the star-catalogue must be due to one of them. Of Agrippa the Arabs knew nothing, while they were well aquainted with the mathematical writings of Menelaus, particularly with his valuable work in three books on spherical geometry. Al Sufi therefore made up his mind that Menelaus was the real author of the star-catalogue.

An attempt to prove that the Arabs must have possessed some information about observations made by Menelaus which is not accessible to us was made in 1901 by the late Dr. Björnbo of Copenhagen.* He founded this on a passage in chapter li. of Al Battani's work on astronomy, in which are given, not only the above-mentioned longitude of β Scorpii for the year A.D. 98, but also longitudes of Regulus and Sirius for the same year, supposed by Björnbo to be taken from some book by Menelaus, and not mentioned by Ptolemy. Björnbo gives the following table:—

	Menelaus.	Ptolemy.	Al Battani.
Sirius	77° 0′	77° 40′	88° 50′
Regulus	122 10	122 30	134 0
Spica	176 15	176 40	[187 50]†
β Scorpii	215 55	216 20	227 45

As regards the two last stars, the difference between Menelaus and Ptolemy is 25′, but the two first stars differ respectively 40′

* "Hat Menelaos aus Alexandria einen Fixsternkatalog verfasst?" *Bibliotheca mathematica*, 3rd Series, ii. pp. 196–212.

† Not given in Björnbo's table; taken from Al Battani's star-catalogue.

and 20′. The conclusion is therefore drawn that Al Battani must have known observations by Menelaus which have not come down to us, and that their number was probably considerable. Still, Björnbo acknowledges that the statement of Al Sufi was only guesswork, and that Al Battani cannot have had before him a catalogue by Menelaus comparable in extent to that of Ptolemy, since he, who placed so much reliance on Menelaus, would then have based his own catalogue on the older catalogue in preference to that of Ptolemy.

But these conclusions are founded on the old and imperfect translation of Al Battani's book by Plato of Tivoli, and they are disproved by the new translation by Nallino, which includes the star-catalogue of Al Battani, not translated before.* The following is a literal translation of the passage in question: † "Una ex observationibus stellarum qua Ptolemæus usus est, fuit observatio quam Menelaus narravit, ex anno 845 dicti regis Nabonassaris; et narravit stellam borealem ex iis quæ sunt inter duos oculos Scorpii fuisse eo anno, cum eam sphæra armillari metitus esset per lunam, in 5° 55′ Scorpii. Et necesse erat, juxta id quod descripsit in libro suo, ut esset Cor Leonis eo anno memorato in $2\frac{1}{6}$° Leonis ac esset Sirius in 17° Geminorum." The subject of "descripsit in libro suo" is Ptolemæus, not Menelaus; and the meaning is, that Al Battani, from the data in Ptolemy's book, estimated that α Leonis in A.D. 98 must have been in 2° 10′ Leonis and Sirius in 17° 0′ Geminorum. He then, from a comparison with his own observations, finds that the longitudes from A.D. 98 to A.D. 880 had increased by 11° 50′. He therefore adds 11° 10′ to Ptolemy's longitudes to form those of his own catalogue of 533 stars. Why he should in one case have subtracted 40′ from Ptolemy's longitude, and in the other case 20′, to form his estimate of the places of the two stars for A.D. 98, is not clear. But it is by no means the only time that there is some confusion or even error in Al Battani's quotations from Ptolemy.

It must therefore be acknowledged that we have no reason to believe that Ptolemy borrowed any star-places from Menelaus, or that the Arabs possessed any knowledge about observations made by him, beyond what we read in the *Syntaxis*. As Menelaus is the only man after Hipparchus of whom it has ever been asserted that he supplied Ptolemy with star-places, we must now inquire whether Ptolemy may not himself have observed *at least* the stars not observed by Hipparchus and possibly others.

That Ptolemy was an observer of the fixed stars is most distinctly stated by himself. In the chapters on precession ‡ he tells us first that the interval between the observations of Hipparchus and the first year of Antoninus, "in which we also have observed most

* *Pubbl. del Reale Osservatorio di Brera in Milano*, No. 40: *Al-Battani sive Albatenii Opus astronomicum*, ed. C. A. Nallino. (3 vols.)

† *Loc. cit.*, ii. p. 270, among notes, compare t. i. p. 124. In the latter place, line 24, there must be a misprint, for 2° 40′ *read* 2° 10′.

‡ *Syntaxis*, vii. 2 and 3, Heib. ii. p. 12 *sqq.*

of the places of the fixed stars," is 265 years. Next, that "when we by observation determined for each star its latitude, we found almost exactly the same values as had been found at the time of Hipparchus, or at least with only small differences which might be due to errors of observation. But when we by observation determined the distances from the equator along the declination circle, we found values differing greatly from those of Hipparchus, while the latter differed very much from those handed down from the astronomers of the school of Timocharis." This is illustrated by giving for eighteen stars the declinations observed by Timocharis, Hipparchus, and Ptolemy. In the introduction to the star-catalogue* the method of determining the longitude and latitude of the stars is set forth. Ptolemy says: "Making use again of the same instrument, whose astrolabe circles turn round the pole of the ecliptic, we have observed as many stars down to the sixth magnitude as it was possible to point to." And he goes on describing in detail how the various circles of the instrument were set, so as to give him the difference of longitude and of latitude of a star already determined and a star under observation. It would be impossible to affirm more distinctly that he has made a large number of observations, though Ptolemy does not say that he has observed every single star in the catalogue. Unless we are prepared to accuse that distinguished mathematician and astronomer of deliberate fraud, it is impossible to maintain that he copied the latitudes of the stars from Hipparchus and merely added 2° 40′ to his longitudes. And now that the discovery of the list of Hipparchian stars renders it at least extremely probable that the catalogue of Hipparchus only contained about 850 stars, even the believers in the unacknowledged use of that catalogue must at any rate credit Ptolemy with the 175 additional stars.

But how are we then to account for the constant error of fully a degree, by which amount Ptolemy's longitudes are too small, as if the epoch of the catalogue was about A.D. 60 and not A.D. 137? Is it possible that Ptolemy's equinox for the year 137 can be affected by so large an error?

We know in a general way how Ptolemy determined the absolute longitude of a standard star. He measured the difference of longitude of sun and moon just before sunset, and as soon as the stars began to appear, the difference of longitude between the moon and a star; and, allowing for the moon's motion during the interval, this gave the difference of longitude between the star and the sun, and consequently the longitude of the star. This was of course only done for a very small number of stars, with which the rest of the stars in the catalogue were then connected. But it is easy to see that considerable systematic errors were introduced in this way. In the only example given by Ptolemy, the observation of Regulus on February 23, A.D. 139, he says expressly that the sun was just setting. Therefore the distance between the sun and the moon must have been diminished by nearly the full amount of

* *Syntaxis*, vii. 4, p. 34 *sqq.*

refraction, about half a degree, and all standard stars observed in this manner and all the catalogue-stars connected with them would be affected by the same error. The interval of time between the two parts of the operation was no doubt made as short as possible in order to eliminate errors arising from imperfect knowledge of the moon's motion in the interval. But Ptolemy had taken over the whole of the solar theory of Hipparchus without alteration; and this was a serious mistake, since the value of the excentricity was too great, which produced an error in the equation of the centre reaching a maximum of $22'$.* Again, the instrumental errors must have been considerable, since the observations did not reveal the fact that the adopted latitude of the Museum of Alexandria, 30° 58′, was 13′ or 14′ too small. Is it any wonder, then, that Ptolemy's equinox was about a degree in error?

But it should also be noticed (which nobody seems to have done hitherto) that observations of the kind just described were not the only ones on which Ptolemy depended when he fixed his equinox. To determine the constant of precession he combines observations made by Timocharis and Hipparchus with later observations in the following manner:—

1. The observation of Regulus made by himself, described above, combined with the place of the star given by Hipparchus.

2. A conjunction of the moon with the Pleiades, observed January 29, 283 B.C., by Timocharis, combined with another observed November 29, A.D. 92, by Agrippa in Bithynia.†

3. An occultation of Spica observed March 9, 294 B.C., by Timocharis, and a conjunction observed by him November 8, 283 B.C., combined with an occultation observed by Menelaus at Rome January 10, A.D. 98.

4. A conjunction of the moon with β Scorpii, observed by Timocharis December 20, 295 B.C., combined with another observed by Menelaus January 13, A.D. 98.

The first observation we have already considered. A worse selection of material for the object in view than the seven conjunctions of the moon with stars it would be difficult to conceive. From these observations Ptolemy deduces the positions of η Tauri, Spica, and β Scorpii for the various years, by calculating the place of the moon by means of his own lunar theory. It must be remembered that the latter was very imperfect. He did not know the lunar variation, of which the maximum effect is 39′, though his "prosneusis" would account for part of this, leaving, however, in the octants errors which might rise to 24′. Neither did he know the annual equation, of which the maximum is 11′. It is, therefore, not surprising that the star-places derived from calculated places of the moon are very poor, and that the resulting constant

* P. Tannery, *Recherches sur l'histoire de l'astronomie ancienne*, p. 171.

† For particulars about these conjunctions see Schjellerup's paper in *Copernicus*, vol. i. (1881), p. 223, and Dr. Fotheringham's paper, *Monthly Notices*, vol. lxxv. (1915), p. 377.

of precession was very erroneous. The following table gives the longitudes thus found compared with the true ones :—

	−293, 294.	−282.	+92.	+98.
	° ′	° ′	° ′	° ′
η Tauri, Ptol.	...	29 30	33 15	...
True	...	28 17	33 29	...
Error	...	−1 13	+14	...
Spica, Ptol.	172 20	172 30	...	176 15
True	172 0	172 10	...	177 25
Error	−20	−20	...	+1 10
β Scorpii, Ptol.	212 0	...	...	215 55
True	211 19	...	...	216 44
Error	−41	...	...	+49

By a curious piece of ill luck the longitudes for the time of Timocharis were all too great and those for the end of the first century too small, which produced the faulty precession of 1° in 100 years. And the longitude of Regulus deduced from the observation described above was 122° 30′, and was therefore 1° 33′ too small, the true longitude for A.D. 139 being 124° 3′. We have no information whatever as to how many standard stars were compared with the sun, nor how often this was done. And, judging from the important part played by conjunctions of stars with the moon in determining the amount of precession, it does not seem improbable that stars may also sometimes have been measured from the moon, and the place of the moon have been taken from Ptolemy's tables, without observing the moon's elongation from the sun. But even if this was never done, the mean of the errors in the adopted longitudes of the four stars mentioned, if we refer them to the year A.D. 137 with Ptolemy's precession, is +63′. This corresponds to the amount of precession in 75½ years, making the real epoch of Ptolemy's catalogue A.D. 62. From 349 zodiacal stars Peters found the epoch to be A.D. 58. Is it then necessary to believe that Ptolemy borrowed his star-places from Hipparchus?

To sum up, the following conclusions may reasonably be drawn :—

1. That the star-catalogue of Hipparchus did not contain more than about 850 stars, so that Ptolemy cannot have borrowed the whole of his catalogue from Hipparchus.

2. That if he did borrow the relative positions of these 850 stars (of which there is no proof), 175 stars, probably identical with the stars observed with an instrument, of which the latitude circle was only divided to ¼°, must at any rate have been observed by Ptolemy.

3. That the assertion of one Arabian astronomer who lived eight hundred years later, that Ptolemy borrowed his catalogue from Menelaus, must be rejected as being totally unfounded.

4. That the methods adopted by Ptolemy for determining the longitudes of standard stars of necessity introduced large systematic errors in the results.

5. That the only four longitudes, of which he gives the values thus determined, are on an average 63′ too small, which agrees perfectly with the average error of the stars in his catalogue.

6. That this is therefore an error in the equinox determined by him; and

7. That there is therefore no reason to disbelieve his positive statement, that he had made extensive observations of fixed stars.

14 *Staverton Road, Oxford:*
1917 *May.*

Thermal Diffusion and the Stars. By S. Chapman, M.A., D.Sc.

It has recently been discovered theoretically,* and verified experimentally,† that a temperature gradient in a mixture of two gases will in general produce diffusion, or a tendency to diffusion. A steady state is possible only when this tendency has become neutralised by its own action in producing a concentration gradient, or variation in the relative proportions of the two gases. The direction of the effect is such that the heavier molecules tend towards the cooler regions, except that if the masses are very nearly equal, and the diameters are very unequal, the larger molecules will pass towards the place of lower temperature.‡ If the masses and diameters (or laws of interaction at collision) are the same for the two gases, thermal diffusion does not occur at all.

It is to be expected that such a phenomenon may have interesting consequences in cosmical physics, especially in the stars, where large temperature gradients are known to exist. The letters which I have received from various friends, containing suggestions or inquiries on this point, have led me to examine the more obvious possibilities. Though the result does not favour the idea that thermal diffusion has very much play in stellar physics, I have thought that it may be of interest to publish a brief note on the matter, in order to make clear why this conclusion seems probable.

The feature which adds special interest to thermal diffusion is that, since the temperature of a star diminishes from centre to surface, the tendency is for the heavier molecules, on this account, to be raised to the higher levels. It is a fact of observation, of a

* Chapman, *Phil. Trans.*, A, ccxviii. p. 1 (received May 19, 1916, but not yet published), and *Proc. Roy. Soc.*, A, xciii. p. 1 (1916). The same phenomenon has also been discovered by rather different mathematical analysis, almost simultaneously, by D. Enskog (*Inaugural Dissertation*, Upsala, 1917).

† Chapman and Dootson, *Phil. Mag.*, March 1917.

‡ *Phil. Mag.*, 1917 (as yet unpublished).

rather remarkable character, that heavy elements *are* observed near the surface of the sun. It is perhaps tempting to connect the two phenomena in a theory of the latter. In the accompanying note on "Convection and Diffusion within Giant Stars," I have discussed the case as regards the interior of a giant star. It appears that the thermal effect is far outweighed by the contrary influence of "pressure diffusion," which tends to produce stratification into layers of increasingly heavy elements towards the centre of the star. The effect of thermal diffusion is in amount only a small percentage of that of pressure diffusion. The reason, as there indicated, is that the concentration gradients which the two actions tend to set up are proportional to the gradients of log p and log T; the constant of proportionality, moreover, being two or three times as great in the former as in the latter case. Within a giant star $p \propto T^n$, where n is probably 4, so that the gradient of log p is four times as great as that of log T.

In the case of a stellar atmosphere, such as that of the sun, the conditions will in many respects differ widely from those prevailing within a giant star. For pressure diffusion to be overcome by thermal diffusion, however (so that heavy molecules may be raised), the exponent n in $p \propto T^n$ would have to be a fraction (say $\frac{1}{3}$), and if Boyle's law is approximately valid, the density ρ would then have to increase outwards according to some such law as $\rho \propto T^{-\frac{3}{2}}$. It is very difficult to suppose that this is the case in fact.

Moreover, even if thermal diffusion were predominant over pressure diffusion, the convective motion as observed is so considerable that it is hardly to be expected that the state of diffusive equilibrium could establish itself. Convection has a powerful mixing effect, and it seems most likely that the presence of elements of widely different weights in the solar atmosphere must be attributed largely to this cause.

Convection and Diffusion within Giant Stars.

By S. Chapman, M.A., D.Sc.

§ 1. Professor Eddington's recent paper * "On the Radiative Equilibrium of the Stars" has given us an altogether new insight into the conditions probably existing within a star, especially within a "giant" star, of low density. He has shown that in the latter case the temperature and pressure gradients towards the centre must be much less than was formerly supposed, the condensing influence of gravity being largely counteracted by the pressure of the outflowing radiation. Definite numerical estimates of various quantities relating to such stars are deducible from his theory, and have proved very successful wherever comparison with

* Eddington, *M.N.R.A.S.*, lxxvii. p. 16, 1916 November.

observational data was possible. The theory may well serve, therefore, as a working hypothesis on which to base the consideration of further problems concerning giant stars. In the present paper this plan is adopted in the attempt to determine the relative importance of convection * and diffusion within a star; this question has, of course, an important bearing on the degree of uniformity of composition in the star.

§ 2. The illustrative model of a giant star which will be considered here is that described as an example in Professor Eddington's paper. The following are its principal data :—

Mass . . .	$2{\cdot}87 \cdot 10^{33}$ grm. (about $1{\cdot}5 \times$ sun)
Radius . . .	$7 \cdot 10^{11}$ cm. (about 10 × sun)
Mean density . .	$0{\cdot}002$ grm. cm.$^{-2}$
Effective temperature	6500°

These correspond to a giant star of spectral type F–G.

The theory of radiative equilibrium indicates that the laws of variation of density ρ, pressure p, and temperature T within the star resemble those occurring in the adiabatic equilibrium of a mass of gas for which the ratio of specific heats (γ) is $\frac{4}{3}$. Therefore

$$p \propto T^4.$$

(If Boyle's law is assumed to hold, also $\rho \propto T^3$.)

The law of variation of T along the radius of a sphere of gas in adiabatic equilibrium has been worked out by Emden (*Gaskugeln*, p. 80) for various values of γ. The following table of values of T, ρ, and p has been deduced from Emden's table 7 (corresponding to $\gamma = \frac{4}{3}$), in combination with Professor Eddington's calculated values of the central pressure (p_0) and temperature (T_0), viz.

$p_0 = 1{\cdot}19 \cdot 10^{12}$ dynes (or $1{\cdot}18 \cdot 10^6$ atmospheres).
$T_0 = 7{\cdot}12 \cdot 10^6$ degrees.

Fraction of Radius.	Temperature.	Density in grm.-cm.$^{-2}$ (Water = 1.)	Pressure in Atmospheres.
0·0	7,120,000°	0·109	1,180,000
0·1	6,580,000	0·086	860,000
0·2	5,340,000	0·046	370,000
0·3	4,000,000	0·0194	118,000
0·4	2,870,000	0·0072	31,000
0·5	2,040,000	0·0026	8,000
0·6	1,360,000	0·00076	1,600
0·7	870,000	0·00020	260
0·8	500,000	0·000038	29
0·9	230,000	0·0000037	1·3
0·98	34,000	$1{\cdot}1 \cdot 10^{-7}$	$6 \cdot 10^{-4}$
1·0	0	0	0

* By convection is meant any motion caused by a pressure gradient, whether or not the latter is due to non-uniform temperature.

For comparison with the densities in the above table, we may note that the density of air at 0° C. and 1 atmosphere pressure is 0·0013.

The details of the above table must naturally not be pressed too far, but they probably give a sound general indication of the conditions existing within the star. One important assumption implied by the table is that the substance of the star is homogeneous, a condition which is the subject of examination in this paper. It will be sufficient for the present, however, to adopt the above numbers as a basis for discussion.

§ 3. The principal calculation to be made will deal with the amount of stratification, or separation of the different constituents of the star into concentric layers, which is to be expected if diffusion is supposed to act undisturbed by any mixing influences. If the results do not agree with observation, we may then conclude that the latter influences are present, and may perhaps infer roughly to what depths they extend. The principal agent tending to restore homogeneity of composition is, of course, convective motion. If the star is sufficiently highly agitated by currents and eddies, the composition may be rendered almost uniform throughout. But we do not know what eddy-producing forces exist within a star, and cannot obtain any direct estimate of their amount. In the case of the sun (which is, however, a dwarf star) we have visible evidence of considerable convection near the surface, but we do not know to what depths this extends. It is a notable fact that in the sun some of the heaviest elements appear in the high levels, *e.g.* lead (atomic weight 207) in the photosphere, and ytterbium (172) even in the chromosphere. The case seems to be similar in some of the giant stars, though the facts regarding a star are less easy to obtain than are those relating to the sun. At the suggestion of the Astronomer Royal, I examined the data for Arcturus, which is of magnitude 0·2 and parallax 0″·066 (Yale), and must therefore be a giant star *: its spectral type is K_0. Adams † has shown that the spectrum of this star resembles that of a sunspot, and he has identified at least 17 elements in it, the one of highest atomic weight being lanthanum (139). As the identification of an element in a star spectrum is more difficult than in the solar spectrum, it seems not unlikely that still heavier atoms will be identified in this and other giant stars. The presence of heavy elements in the outer layers, alongside those of much smaller atomic weight, is a remarkable fact, and one which requires explanation.

§ 4. We will first consider the state of affairs which would be brought about by diffusion alone, assuming that there is no

* Its light emission must be about 300 times that of the sun, so that if the surface luminosity is similar in the two cases, Arcturus must be of large radius and presumably of low density.

† Adams, *Ap. J.*, xxiv. p. 69.

relative motion in the star. The causes which are effective in producing differences of composition are—

(*a*) external forces acting unequally per unit mass on the different constituents;
(*b*) a gradient of pressure;
(*c*) a gradient of temperature.*

The direction in which (*a*) will act is obvious; (*b*) will cause the heavy elements to tend towards regions of higher pressure; while (*c*) will cause the heavier molecules to move towards the cooler regions, except that if two sets of molecules are nearly alike in mass, and of widely different diameters, the larger ones will tend towards the cooler regions.† The latter case, however, hardly concerns us in the present instance.

In the star under consideration, the external forces acting on the molecules are those of gravity and radiation pressure. If the atoms are split up into positive and negative particles at the centre of the star, owing to the high temperature prevailing there, electrical forces would also come into play which might be considerable in certain circumstances. These will not be considered here, however.‡ As regards the two forces just mentioned, gravity acts proportionately to the mass of each molecule, and we may assume, for the time being, that so also does radiation pressure. They will consequently not produce diffusion.

In the mathematical treatment of the other two agents, (*b*) and (*c*), it is convenient to idealise the problem by supposing that only two gases are present, of molecular weights m_1 and m_2 (say $m_1 > m_2$), and relative proportions, by volume, λ_1 and λ_2 (so that $\lambda_1 + \lambda_2 = 1$). The equation of steady state, as regards (*b*) and (*c*), may be written

$$\frac{\partial \lambda_1}{\partial x} = -\frac{\partial \lambda_2}{\partial x} = k_p \frac{\partial \log p}{\partial x} - k_T \frac{\partial \log T}{\partial x},$$

where k_p and k_T are certain diffusion factors which are defined in the memoir already cited. The gradient of relative concentration (λ_1 or λ_2) is therefore compounded of two terms proportional respectively (over ranges in which k_p and k_T are nearly constant) to the gradients of log p and log T. In the case of the star in question,

$$p \propto T^4, \quad \log p = \text{constant} + 4 \log T;$$

* The last agent, which I have termed thermal diffusion, appears to have remained unknown until published, as a theoretical discovery, in *Phil. Trans.*, A, ccxvii. p. 1 (received 1916 May 19). An abstract is given in *Proc. Roy. Soc.*, A, xcviii. p. 1. Enskog (*Inaugural Dissertation*, Upsala, 1917) has also discovered the same phenomenon, almost simultaneously. Some experiments confirmatory of the theory are given in *Phil. Mag.*, 1917 March (Chapman and Dootson).

† *Phil. Mag.*, July 1917.

‡ They would, of course, be such as to oppose any general separation of the opposite charges.

so that, other things being equal, thermal diffusion will not be so powerful an agent as pressure diffusion. Moreover, except when the molecular masses are nearly equal, k_p seems to be twice or thrice as great as k_T, so that the concentration gradient due to the pressure effect will be ten or twelve times as large as that due to the temperature gradient. The two effects are in opposition in the giant star: the rise of temperature towards the centre neutralises, to the extent of perhaps about 10 per cent., the central condensing influence which the rise of pressure exerts on the heavier gas. This conclusion, it should be noted, does not depend on the actual magnitudes of the pressure and temperature gradients, but only on the relation $p \propto T^4$. If the star were in adiabatic equilibrium, and $\gamma > \frac{4}{3}$, p will vary according to a lower power of T: thus for a monatomic gas, $\gamma = \frac{5}{3}$, and the corresponding relation between p and T is $p \propto T^{\frac{5}{2}}$. In such a case thermal diffusion would be more effective than in the above instance: perhaps something of this kind may occur in a dwarf star, where radiation pressure is relatively unimportant compared with gravitation.

The constant k_p is given by

$$k_p = \lambda_1\lambda_2 \frac{m_1 - m_2}{\lambda_1 m_1 + \lambda_2 m_2},$$

and k_T also contains the factor $\lambda_1\lambda_2$, though the remainder of the expression cannot be written down in simple terms. We shall incorporate the effects of thermal and pressure diffusion in one equation, for simplicity, taking $k_p = \frac{5}{2}k_T$ and $p \propto T^4$. Then we have

$$\frac{\partial \lambda_1}{\partial x} = -\frac{\partial \lambda_2}{\partial x} = \frac{18}{5} k_p \frac{\partial \log T}{\partial x}$$

as our equation of composition.

§ 5. This will now be applied to the problem of a star composed of two elements, one of which is four times as heavy as the other. The lighter one, for instance, may be thought of as iron (56) and the other as lead (207) or radium (226). In this case $k_p = \frac{7}{2} \frac{3\lambda_1\lambda_2}{1 + 3\lambda_1}$. Writing the equation of composition in the form

$$\frac{\partial \log \lambda_1}{\partial x} = \frac{7}{2} \frac{3\lambda_2}{1 + 3\lambda_1} \frac{\partial \log T}{\partial x}$$

we see that

$$\lambda_1 \propto T^{\frac{7}{2} \frac{3\lambda_2}{1+3\lambda_1}}$$

over a range throughout which the index is nearly constant. If the proportion of the heavy element is large, it changes only very slowly. If, on the other hand, it is very small, $\lambda_1 \propto T^{10\cdot5}$. Suppose, for instance, that at the centre of the star there are 9 parts by volume of the heavy element to 1 of the light element. This proportion will at first vary very gradually: a rough calcula-

tion indicates that the proportions at other fractions of the total radius will be as follows, if we neglect the change in the pressure and temperature gradients caused by the varying composition along the radius :—

r (Fraction of Total Radius).	T.	ρ.	λ_1.	λ_2 (to 3 Decimal Places).	Fraction of Mass within Radius r.		
					(*a*)	(*b*)	(*c*)
·000	7,120,000	0·109	0·900	0·100	0·000	0·000	0·000
·109	6,500,000	0·083	0·877	0·123	0·060	0·076	0·078
·217	5,120,000	0·040	0·790	0·210	0·312	0·380	0·397
·290	4,150,000	0·022	0·675	0·325	0·519	0·599	0·635
·362	3,290,000	0·0107	0·519	0·481	0·695	0·749	0·811
·435	2,560,000	0·0050	0·316	0·684	0·820	0·825	0·912
·507	1,970,000	0·0023	0·120	0·880	0·903	0·850	0·960
·580	1,490,000	0·0010	0·018	0·982	0·951	0·854	0·982
·652	1,110,000	0·00041	0·0010	0·999	0·979	0·855	0·993
·725	790,000	0·00015	·000034	1·000	0·994	0·855	0·999
·870	310,000	0·00001	$2·1 . 10^{-9}$	1·000	0·999	0·855	1·000

According to this table, the heavy gas will practically not extend further from the centre of the star than three-quarters of the total radius. This result, however, certainly much overstates the outward extension of the heavy gas. In the last three columns of the table are given the proportions of the total mass, within radius r, (*a*) in the case of a homogeneous star, the only case in which the above values of T and ρ are strictly correct (this column has been calculated from the table by Emden already mentioned), and (*c*) in the present case. Column (*b*) gives the proportion of the mass of the heavy gas within radius r to the total mass of the star. The greater concentration of mass towards the centre will, however, make the pressure gradient greater than that which corresponds to a homogeneous star, and this will increase the central condensation of the heavy gas: thus, if we consider a star composed of 85 per cent. of the heavy gas to 15 per cent. of the lighter gas (reckoning by mass), there will be more than 90 per cent. of the heavy gas at the centre (by volume), while at a central distance of six- or seven-tenths of the radius there will be only a very small fraction of 1 per cent. of the heavy constituent.

Since it does not seem probable that in an actual star there would be so large a proportion (by mass) of the heavy constituents as 85 per cent., I have also calculated a similar table, as follows, starting from the hypothesis that at the centre of the star the proportion of heavy gas, by volume, is $\frac{2}{3}$:—

r.	T.	ρ.	λ_1.	λ_2.	Fraction of Mass within r. (*a*)	(*b*)	(*c*)
·000	7,120,000	0·109	0·667	0·333	0·000	0·000	0·000
·036	7,050,000	0·106	0·660	0·340	0·003	0·005	0·006
·072	6,830,000	0·096	0·628	0·372	0·019	0·037	0·042
·109	6,500,000	0·083	0·555	0·445	0·060	0·108	0·125
·145	6,080,000	0·068	0·427	0·573	0·125	0·205	0·247
·181	5,610,000	0·054	0·228	0·772	0·212	0·290	0·376
·217	5,120,000	0·040	0·0465	0·954	0·312	0·331	0·481
·254	4,630,000	0·030	0·0011	0·999	0·417	0·339	0·565
·290	4,150,000	0·022	$3·7 . 10^{-5}$	1·000	0·519	0·339	0·640
·362	3,290,000	0·0107	$1·2 . 10^{-8}$	1·000	0·695	0·339	0·772
·435	2,560,000	0·0050	$2·4 . 10^{-13}$	1·000	0·820	0·339	0·867
·652	1,110,000	0·00041	...	1·000	0·979	0·339	0·984
·870	310,000	0·00001	...	1·000	0·999	0·339	0·999

As the table shows, in this case the proportion, by mass, of the heavy constituent is approximately one-third, and this is almost entirely concentrated within the central region of one-quarter the total radius, while all traces of it have vanished at the half radius. As before, these figures underestimate the centralisation of the heavy gas, owing to the more rapid pressure gradient which would result from the heterogeneity of composition.

The proportion, by mass, of the heavy gas is still large in the last example, viz. one-third, and the difference of concentration between the two cases indicates how greatly the concentration increases as the proportion diminishes. If, for instance, only one-tenth of the mass of the star were due to the heavy substance, this would settle down completely within a radius of one-fifth, or less.

In an actual star there would be, not merely two elements, but a large number of different ones with molecular weights extending over a wide range. It is difficult to discuss this case in detail, since we know so little about the relative amounts of the different substances. The result of the graded nature of the mixture will be a less sharp separation between the layers of different composition. As a rough approximation we may take the elements in groups covering a range of 50 or 80 in atomic weight. The mass ratio between two groups will be less than in the above examples, and to illustrate the effect of this we will consider a further example in which there are two gases (only) with a mass ratio of 2 in place of 4. We will also suppose that there is one part in four, by volume, of the heavy gas at the centre of the star. This is found to correspond to a proportion, by mass, in the whole star, of 0·258 of the heavy gas. The proportions by volume at different distances are as follows:—

Distance r	0·000	0·072	0·145	0·217	0·290	0·362	0·435	0·507
λ_1	0·250	0·225	0·126	0·024	0·0009	$6 . 10^{-5}$	$1·6 . 10^{-6}$	$2 . 10^{-8}$

Thus the heavy element practically does not appear beyond the half radius. In the former case, however, with a larger amount of the heavy gas, the centralisation was greater.

As regards the light gases like hydrogen, these should, of course, be on the surface in rather sharply defined layers. If we consider the two elements iron and hydrogen, m_1/m_2 is about 50, and when λ_2 is nearly 1 the value of λ_1 will be proportional to T^{170} approximately. In the model star considered, the temperature gradient near the surface is comparable with the maximum gradient (one-sixth, in fact), so that the iron layer should thin out very rapidly at its upper surface. The same is true, to a slightly less extent, for the lighter elements such as magnesium (24).

§ 6. The above calculations are only roughly illustrative of the conditions which would exist in a star if diffusion could act undisturbed by mixing influences, but they indicate a degree of stratification according to density which seems to be incompatible with the observed presence of heavy elements at the surface of the stars. It is therefore necessary to consider what explanation can be given for their appearance in the upper levels.

One possibility is that radiation pressure does not act proportionately to the mass of the atoms, but is more effective per unit mass on the heavy than on the light elements. The facts on this point are not known; should experimental data become available, it would be an easy matter to take account, in this calculation, of any observed departure from the assumed law, but it has not seemed worth while to consider hypothetical cases of this kind.

The most likely explanation would seem to lie in the presence of convective motion, which, to be effective in raising heavy elements from their natural (diffusion) levels towards the surface, must extend inwards to a depth of one-half or one-third the radius of the star. The degree of convection necessary to counteract the diffusive tendency to stratification is hardly a matter which can be the subject of calculation at present. The case can be illustrated, however, by consideration of what takes place in the earth's atmosphere. There the pressure gradient (p diminishing upwards) should produce by diffusion a change of 1 per cent. in the proportions of oxygen and nitrogen in every 5 kilometres: the temperature gradient would neutralise this tendency to the extent of a few per cent. of its whole amount. But careful tests have revealed no such change of composition up to a height of 15 kilometres, a circumstance which is natural in view of the continual agitation of the troposphere by convection currents. In the stratosphere or isothermal layer, however, it is generally supposed that convection is absent, so that the various constituents of the atmosphere settle out more or less definitely into layers according to their molecular weights.

The absence of convection in the upper air is referable partly to the diminution of disturbing forces and also to the fact that the upper air is much more viscous, in proportion to its mass, than that of the troposphere. The viscosity μ of a gas is independent

of the pressure p or density ρ (the temperature being kept constant) down to very low pressures. Hence the kinematic viscosity μ/ρ, on which depends the damping of differential motions such as occur in eddying masses of gas, increases inversely as the density. At the earth's surface it is 1·3 C.G.S. units; at a height of 100 kilometres it is about one million times as great.

The independence of μ on p fails both for very low and very high pressures; in the case of carbon dioxide* at pressures of 60 atmospheres, or more, μ seems to vary as p^n, where n is 2 or even more. Also μ varies with temperature, as T^m, where m seems always to lie between $\frac{1}{2}$ and 1. In the case of a star, if these laws hold good under the conditions found there, $\mu \propto p^2 T^{\frac{1}{2}}$ at least, *e.g.* as $T^{9/2}$. Hence $\mu/\rho \propto T^{3/2}$, and the kinematic viscosity should increase to a high value towards the centre of the star. The disturbing forces tending to produce eddies are probably greater near the surface than far within the star, so that we may expect that convective mixing will be least important in the innermost regions. Again, the time taken to bring about the steady state by diffusion is proportional to 1/D, where D is the coefficient of diffusion. Under ordinary conditions D is a small multiple of the kinematic viscosity μ/ρ, and varies approximately in the same manner. Hence if μ/ρ increases towards the centre of the star, not only will any tendency to convective mixing be hindered, but the process of diffusion will be accelerated. We may therefore expect that a certain amount of settling-out of the heaviest substances takes place near the centre, but that convection extends to a very considerable depth within the star, sufficiently to raise many heavy elements to the surface layers.

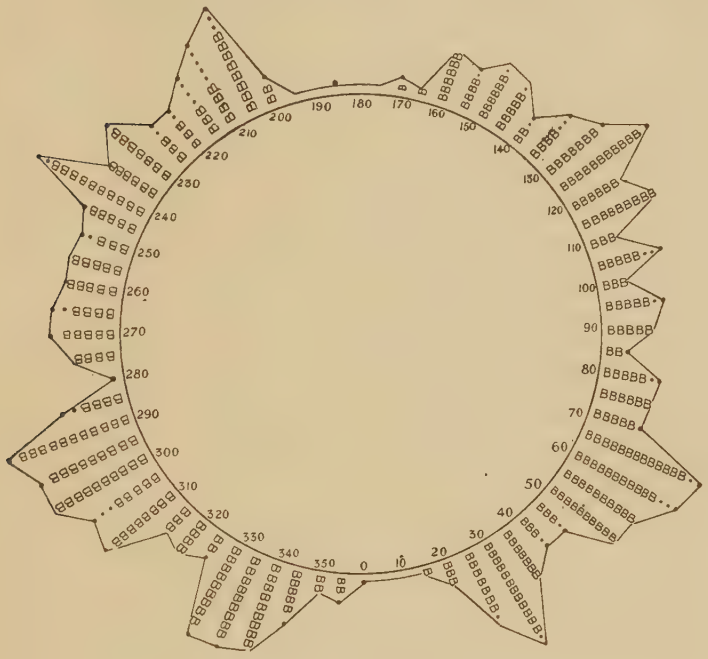
[*Note added* 1917 *May* 30.] § 7. The above calculations would to a large extent lose their point should it prove that the substance of the interior of the star is not in the physical condition which is there contemplated. It may be the case, for instance (as Mr. Jeans suggested in his recent Bakerian lecture, 1917 May 17), that at the high temperatures in the interior all molecules become split up into their component electrons, so that differences of molecular weight or constitution cease to be. The ordinary chemical elements would then occur only in an upper layer, of unknown thickness, where the temperature is sufficiently low to permit of their continued existence. If so, the above calculations would not begin to apply until this layer was reached; the conclusions reached would probably be modified so that a smaller demand would be made on such mixing agencies as convection, to account for the presence of heavy elements in the surface layers.

* *Cf.* Jeans' *Dynamical Theory of Gases*, 2nd ed., p. 297.

Solar Prominences, 1916. By G. J. Newbegin.

The weather conditions of the months January and December were such that in the course of the year no more than sixty-three observations were recorded.

For enumeration on the plan of the International Solar Union,



Distribution of Prominences, 1916.

in which the lower limit of height is taken as 30″, 343 prominences are available, which, when plotted into diagrammatic form, affect 425 position-angles.

The diagram indicates a growth in activity all round, the polar regions only showing a want of occupation, though they are much more filled up than in 1915.

A general trend of activity towards the northern hemisphere is noticeable.

The dark absorption has been observed on thirty occasions, on 5 faint and 25 bright prominences.

The general brightness has increased, and faint prominences are proportionally fewer.

Observations of (Ch. 7085) RT Cygni in 1912–17.

By A. N. Brown, M.A.

I began to observe this Long-Period Variable, the Harvard designation of which is 194048, in 1908, the observations, with light curves, down to the beginning of 1912, having been printed in *Monthly Notices*, vols. lxix.–lxxii. The 212 observations which follow continue the series, and, as I have not previously shown the dates of maxima and minima in tabular form, they are now given from 1908 to the beginning of this year. It has not been thought necessary to reproduce the curves drawn from the later observations.

Maxima.			Minima.		
Date.	Julian Date.	Mag.	Date.	Julian Date.	Mag.
			1908 Apr. 2	8034	11·1
1908 June 29	8122	7·3	1908 Oct. 11	8226	11·3
1909 Jan. 2±	8309	7·7	1909 Apr. 24	8421	11·1
1909 July 20	8508	7·2	1909 Oct. 31	8611	11·0
1910 Jan. 20	8692	6·6	1910 May 6	8798	10·7
1910 July 17	8870	7·1	1910 Nov. 24	9000	11·5
1911 Feb. 17	9085	8·1	1911 May 22	9179	11·2
(1911 Aug. 21)	(9270)	...	1911 Nov. 21	9362	11·2
1912 Feb. 4±	9437	7·8	1912 June 5	9559	11·2
1912 Sept. 2	9648	7·8	1912 Dec. 14	9751	10·9
1913 Feb. 23	9822	6·5	1913 June 28	9947	10·9
1913 Sept. 18	0029	7·8	1913 Dec. 25	0127	10·9
1914 Mar. 16	0208	7·0	1914 July 9	0323	11·3
1914 Oct. 8	0414	7·5	(1915 Jan. 21)	(0519)	...
(1915 Apr. 10)	(0598)	...	1915 July 20	0699	11·1
1915 Oct. 16	0787	7·4	1916 Feb. 9±	0903	11·3
1916 May 6	0990	7·0	1916 Aug. 17	1093	10·9
1916 Nov. 4	1172	7·3	1917 Feb. 16	1276	11·2

The symbol ± above implies that the date is somewhat uncertain owing to paucity of observations, while the dates given in brackets are conjectural, the observations being interrupted at these periods. But in the case of none of the phases indicated above, even where the curve is most pointed, is it strictly accurate to speak of the maximum or minimum as having occurred definitely on any particular day, since this star, as indeed is the case with all Long Periods with which I am familiar, always remains

at maximum and minimum light during several days. On assigning the above dates, therefore, I have endeavoured to give the day when the middle point of each phase had been reached. Neither at maximum nor minimum has the curve any fixed shape. Sometimes it is more or less rounded, sometimes comparatively flat. But the rise is invariably more rapid than the fall, on the average by 27 days. The period as deduced from these observations is 192·4 days. The Harvard period is 190·5 days.

By far the most remarkable of the maxima is that which is assumed to have occurred on 1915 Oct. 16. If the observations between Sept. 19 and Dec. 12 in this year are plotted, it will be seen that there was a rise to a maximum of 7·4 mag. on Oct. 16; a fall to 7·7 mag. on Nov. 2; and again a rise, this time to 7·3 mag., on Nov. 17. After this the light faded quickly, the estimate for Nov. 26 being 8·0 mag. Here then seems to be a clear case of a double maximum. M. Félix de Roy, a member of the Variable Star Section of the B.A.A., has kindly allowed me to compare his observations made at this time with mine. On the whole the two series agree well, and M. de Roy's—he considers the date of maximum to have been Oct. 18—certainly show indications of a fall on about Nov. 4 which continued for several days and was followed by an abrupt rise. But unfortunately, just at the period when I think the second maximum occurred, his observations are somewhat scanty in number. As the date in October corresponds much more closely with the mean period of the Variable, I have entered it in the table given above.

In 1911 I noted (*M.N.*, vol. lxxii. p. 417) a case of a very faint maximum (8·1 mag.) being preceded by an unusually slow fall to minimum. In subsequent years so far there has been no minimum as faint as 8·1 mag. The three faintest are assumed to have occurred on 1912 Feb. 4 ±; 1912 Sept. 2; and 1913 Sept. 18, all of 7·8 mag. The maximum immediately before the first of these was not observed, but the falls to the minimum which preceded each of the other two took 122 ± and 125 days respectively, the average time being 109·6 days. It almost seems as if the energy of the Variable is to some extent dissipated during these slower falls to minimum, so that it is incapable of rising to its normal maximum brilliance.

The arrangement of the different columns showing the observations is the same as that adopted in my observations of V Cassiopeiæ which were published last year in *M.N.*, lxxvi. pp. 606–612, and is there fully explained; but in Hagen's Atlas (series iv.) Harvard (H.P.) magnitudes are given for only some of the comparison stars for RT Cygni. I have adopted these values for each comparison star which I have used when they are there given, and in all other cases have employed Hagen's own magnitudes.

The telescopes used were 5-inch and 3-inch refractors with powers varying from 25 to 200.

The Observations.

Date.	Julian Date.	Comparisons.	Ded. Mag.
1912			
Jan. 26	9428	5−1·5, 7+1	7·9
27	9429	5−1·5, 7+0·5	8·0
29	9431	5−1, 7+1	7·9
Feb. 2	9435	5+1	7·7
4	9437	4−2·5, 6+0·5; 5+1·5	7·8
Apr. 3	9496	14−2, 22+6	8·9
29	9522	41+1	10·3
May 24	9547	=44	11·0
June 2	9556	44−1·5	11·2
8	9562	44−2	11·2
10	9564	44−2	11·2
20	9574	44+1	10·9
22	9576	43−3, 44+1	10·9
28	9582	41−1·5, 43+0·5	10·6
July 4	9588	41+2	10·2
11	9595	22−7, 41+2	10·2
14	9598	25−1	9·8
16	9600	23−1·5, 25+1	9·6
25	9609	14−3, 18+1; 15−3·5, 20+2; 16−4, 19+2	9·1
30	9614	15−1, 20+3·5; 16−1·5, 19+4·5	8·9
Aug. 1	9616	15+1; 16−1	8·8
29	9644	4−1·5, 6+1	7·8
30	9645	4−1·5, 6+1	7·8
Sept. 2	9648	4−1·5, 6+1	7·8
17	9663	5−1·5, 7+0·5	8·0
20	9666	5−1·5, 7+0·5	8·0
23	9669	5−2, 7+0·5	8·0
Oct. 2	9678	7−3, 10+1	8·3
3	9679	6−3·5, 10+1	8·3
9	9685	10−2, 14+1	8·6
10	9686	13+1	8·4
14	9690	14−1, 19+5; 15+1	8·8
17	9693	14−1·5, 19+5	8·8
Nov. 3	9710	22−1; 23−0·5, 25+1	9·6
29	9736	41−1·5, 43+0·5	10·6
Dec. 10	9747	43−3, 44+1	10·9
12	9749	43−3, 44+1	10·9
16	9753	43−2·5, 44+1	10·9
18	9755	43−3, 44+1	10·9
29	9766	41−1·5, 43+1	10·5
1913			
Jan. 1	9769	41+1·5	10·3
5	9773	30−1·5, 41+3	10·1
12	9780	23−1·5, 25+1	9·6
26	9794	6−5, 12+0·5; 10−1, 13+0·5; 14+2	8·5
Feb. 2	9801	7−3, 10+1	8·3
11	9810	3−7, 4+2	7·4
16	9815	2−1, 3+0·5	6·6
18	9817	2−1, 3+0·5	6·6
20	9819	2−1, 3+0·5	6·6
22	9821	2−0·5, 3+1·5	6·5
Mar. 2	9829	2−1, 3+0·5	6·6
11	9838	3−6, 4+4	7·3
30	9857	5−1, 7+1	7·9
May 1	9889	14−2, 19+4; 15−1, 20+4	8·9
6	9894	14−4·5, 19+1·5; 15−3, 20+1·5	9·1
10	9898	18−1, 19+1	9·2
13	9901	15−4, 19+1	9·2
16	9904	19−1·5, 22+1	9·4
24	9912	30−2·5, 41+1	10·3
30	9918	41−0·5, 43+1	10·5
June 2	9921	41−1, 43+1	10·5
5	9924	41−1, 43+0·5	10·5
7	9926	41−1·5, 43+0·5	10·6
23	9942	43−2·5, 44+1	10·9
27	9946	43−3, 44+1	10·9

The Observations—continued.

Date.	Julian Date.	Comparisons.	Ded. Mag.
1913			
July 3	9952	43−2·5, 44+1	10·9
11	9960	41−1·5, 43+0·5	10·6
13	9962	41−1·5, 43+0·5	10·6
20	9969	25−2·5, 30+1	9·9
24	9973	=22	9·5
30	9979	15−1·5, 20+3·5; 16−2, 19+4	8·9
Aug. 28	0008	3−5, 4+5	7·2
Sept. 6	0017	3−8, 4+2	7·5
7	0018	3−7, 4+2	7·4
15	0026	4−0·5, 6+2	7·7
24	0035	3−6·5, 4+3	7·3
28	0039	3−7, 4+2	7·4
Oct. 6	0047	4−1, 6+2	7·7
Nov. 5	0077	14−4·5, 19+2	9·1
19	0091	19+1	9·2
22	0094	30−2, 41+2	10·2
28	0100	41+1	10·3
Dec. 14	0116	43−3, 44+1	10·9
18	0120	43−3, 44+1	10·9
28	0130	43−3, 44+1	10·9
30	0132	43−3, 44+1	10·9
1914			
Jan. 3	0136	43−2·5, 44+1	10·9
5	0138	43−2·5, 44+1	10·9
23	0156	=30	10·0
26	0159	25−2, 30+1	9·9
Feb. 15	0179	7−5, 14+1·5	8·5
22	0186	3−7, 4+2	7·4
28	0192	3−5, 4+5	7·2
Mar. 21	0213	3−3, 4+6·5	7·0
27	0219	3−3, 4+6·5	7·0
31	0223	3−7, 4+2	7·4
Apr. 25	0248	5−0·5, 7+1·5	7·9
27	0250	7−1, 10+3	8·1
May 15	0268	15−4, 20+1; 16−5, 19+1	9·2
17	0270	15−4, 20+1; 16−5, 19+1	9·2
1914			
May 21	0274	=22; =23	9·5
24	0277	25−1	9·8
26	0279	=25	9·7
June 4	0288	41−0·5, 43+1·5	10·5
14	0298	43−2·5, 44+1	10·9
21	0305	=44	11·0
23	0307	=44	11·0
28	0312	44−2	11·2
July 4	0318	44−2	11·2
9	0323	44−2·5	11·3
17	0331	44−2	11·2
29	0343	44−1	11·1
Aug. 14	0359	41−0·5, 43+1·5	10·5
26	0371	14−4·5, 19+2	9·1
29	0374	14−4·5, 19+2; 15−3, 20+1·5	9·1
Sept. 5	0381	7−3, 10+1	8·3
8	0384	5−1·5, 7+0·5	8·0
11	0387	5−1, 7+1	7·9
19	0395	4−2, 6+1; =5	7·8
24	0400	4−1, 6+2	7·7
29	0405	=4	7·6
Oct. 5	0411	3−8, 4+2	7·5
10	0416	3−8, 4+2	7·5
18	0424	4−1, 6+1	7·8
Nov. 1	0438	7−3, 10+1	8·3
16	0453	14+1	8·6
18	0455	10−2, 14+0·5	8·6
Dec. 1	0468	16−3·5, 19+2·5	9·1
10	0477	23−1·5, 25+1	9·6
16	0483	25−1·5, 30+1·5	9·9
1915			
Jan. 5	0503	43−2, 44+1·5	10·8
11	0509	43−3, 44+1	10·9
17	0515	=44	11·0
Apr. 18	0606	3−4, 4+6·5	7·0
23	0611	3−2·5, 4+7·5	6·9
29	0617	3−6·5, 4+3	7·3
May 7	0625	3−6·5, 4+3	7·3

The Observations—continued.

Date.	Julian Date.	Comparisons.	Ded. Mag.
1915			
May 23	0641	10−1, 14+2	8·5
30	0648	14−1·5, 19+5	8·8
June 6	0655	14−4·5, 19+1·5	9·1
15	0664	=19	9·3
9	0688	43−1·5, 44+3	10·7
15	0694	44−1	11·1
July 26	0705	43−3, 44+1	10·9
28	0707	43−2·5, 44+1	10·9
Sept. 11	0752	14−5·5, 19+0·5	9·2
19	0760	14−1; =16	8·7
25	0766	7−1, 10+2·5	8·1
29	0770	5−0·5, 7+1·5	7·9
Oct. 4	0775	=4	7·6
14	0785	3−7, 4+2	7·4
18	0789	3−7, 4+2	7·4
Nov. 2	0804	4−0·5, 6+2	7·7
5	0807	=4	7·6
9	0811	3−8, 4+2	7·5
14	0816	3−6·5, 4+3	7·3
17	0819	3−6, 4+4	7·3
26	0828	=7	8·0
Dec. 8	0840	14−1, 19+5·5	8·8
12	0844	14−5, 19+1·5	9·2
1916			
Jan. 5	0868	43−2·5, 44+1	10·9
8	0871	43−3, 44+1	10·9
13	0876	43−3, 44+0·5	10·9
22	0885	44−1	11·1
Feb. 6	0900	44−3	11·3
Apr. 2	0956	10−1·5, 14+1·5	8·5
9	0963	5−1, 7+0·5	7·9
14	0968	4−1, 5+0·5	7·7
May 3	0987	3−5, 4+5	7·2
15	0999	3−3, 4+6·5	7·0
19	1003	3−7, 4+2	7·4
28	1012	4−1, 5+0·5	7·7
June 6	1021	=5	7·8
21	1036	14−4·5, 19+1·5	9·1
30	1045	=25	9·7

Date.	Julian Date.	Comparisons.	Ded. Mag.
1916			
July 8	1053	43−1, 44+2·5	10·7
22	1067	=43	10·6
25	1070	43−3, 44+1	10·9
Aug. 8	1084	43−3, 44+1	10·9
19	1095	=44	11·0
27	1103	43−2·5, 44+1	10·9
Sept. 13	1120	22+1	9·4
21	1128	15−4, 20+1; 16−4·5, 19+1·5	9·2
27	1134	15−2·5, 20+2·5	9·1
Oct. 15	1152	3−7, 4+2	7·4
19	1156	3−8, 4+2	7·5
21	1158	3−7, 4+2	7·4
24	1161	3−7, 4+2	7·4
28	1165	3−6, 4+4	7·3
31	1168	3−6·5, 4+3	7·3
Nov. 2	1170	3−7, 4+2·5	7·4
8	1176	3−6·5, 4+3	7·3
12	1180	3−6·5, 4+3	7·3
16	1184	3−5, 4+5	7·2
20	1188	3−7, 4+2	7·4
24	1192	3−3, 4+6·5	7·0
26	1194	3−5, 4+5	7·2
Dec. 3	1201	4+1	7·5
25	1223	14−3, 19+3	9·0
27	1225	14−4·5, 19+1·5	9·1
30	1228	14−5, 19+1	9·2
1917			
Jan. 4	1233	=19; 22+1	9·4
10	1239	25−2, 30+1	9·9
27	1256	Glimpsed. ∠41	∠10·4
Feb. 7	1267	44−1	11·1
13	1273	44−2	11·2
26	1286	=44	11·0
Mar. 7	1295	43−3, 44+1	10·9
16	1304	41+1	10·3
22	1310	=41	10·4
27	1315	30−2·5, 41+1	10·3
Apr. 15	1334	13−1, 14+0·5	8·6

The Comparison Stars Used.

Hagen's Number.	Magnitude. H.P.	Magnitude. Hagen.	Hagen's Number.	Magnitude. H.P.	Magnitude. Hagen.
2	6·50		16	8·71	
3	6·67		18		9·1
4	7·64		19		9·3
5		7·8	20		9·3
6		7·9	22		9·5
7		8·0	23		9·5
10	8·39		25		9·7
12		8·5	30		10·0
13	8·54		41		10·4
14	8·67		43		10·6
15		8·8	44		11·0

Ludgrove, New Barnet, Herts:
1917 *May* 2.

Baxendell's Observations of Variable Stars. Ninth Instalment. No. 23, S Vulpeculæ, and No. 24, S Aquilæ; with notes on No. 13, T Herculis, and No. 18, R Serpentis. Edited by Professor H. H. Turner and Miss Mary A. Blagg. (Plate 7.)

No. 23. *S* (2) *Vulpeculæ* (7106 *Chandler*).
(No. 1773 Greenwich 12-year Cat. 1840.)

$19^h\ 42^m\ 27^s\ +26^\circ\ 55'{\cdot}7$ (1855·0)
19 44 18 +27 2 ·3 (1900·0)

The Comparison Stars.—For a long time no chart of the comparison stars could be found, and application was made to Mr. Joseph Baxendell, "junior," for help in clearing up the matter. He suggested that the name "Greenwich 1773," which was in use near the time of discovery, might be on the chart; and with this clue a chart was found among some papers which had not been identified. It is labelled simply "26° 57′ North 1773," particulars which are amply sufficient to identify it when once a clue is furnished, but which, in the absence of any R.A.; of any interpretation of the number 1773; of any scale of magnitude for the stars charted, or of length for their positions; and, finally, of any indication which side is *p* or *f*, and whether north is up or down,—was, as it stood, a serious puzzle, only solved by Mr. J. Baxendell's further kind help.

The difficulties will be better understood by a glance at this very interesting chart, which is reproduced on Plate 7. It is natural to look first for the identification of the stars with the smaller numbers attached, seeing that on almost any principle of numbering the brighter stars would be numbered first. But the stars numbered 4 and 5, for instance, are actually so faint that they are not marked on Father Hagen's chart (in the A.S.V.) at all. What increased the trouble was that (although star 4 was never used) star 5 was nominally used once by Baxendell, and from this single observation a magnitude 8·6 can be fairly inferred. A star so bright would certainly have been on the A.S.V. chart. It seems clear that the "5" of 1866 Nov. 3 must be a simple mistake for "8."

When, however, a few stars had been identified by Mr. Baxendell "junior," it was seen at once that the chart was very accurately made, and must represent many hours of careful work at the telescope. Most of the stars are included in the *Oxford Astrographic Catalogue* (though they are divided between two plates with centres $19^h\ 40^m\ +27°$ and $19^h\ 48^m\ +27°$), and it was easy to compare the places there given with Baxendell's positions as measured from his chart. For this comparison measures of the chart were made with a millimetre scale to a tenth of a millimetre, by naked eye, without magnification. The scale of the chart is apparently 1600″ to 3 inches, so that 1 mm. = 21″, and 0·1 mm. = 2″·1. It is possible that the chart is worth measuring under a microscope, but the figures of Table I. suggest that it would be very difficult to judge whether any observed displacements are due to errors in the chart or to movements in the stars.

Baxendell's No. 7 is just outside the réseau on this plate, but is measurable with quite sufficient accuracy, and the Oxford measures have been added in brackets.

Baxendell's No. 22 is so faint that it was not noticed in the measuring, and can only be seen with difficulty. Measures have however, been made, and added.

Attention is attracted by the large residuals for Baxendell's Nos. 18 and 37. Is it possible that these represent real displacements since 1861? Fortunately we have them on two plates of the *Oxford Astrographic Catalogue*,

No. 1112, centre $19^h\ 48^m\ +27°$: exposed 1897 Sept. 3,
and No. 2389, centre 19 44 +26 : exposed 1904 Oct. 3,

so that there is an interval of seven years (one-fifth of the interval from Baxendell's epoch 1861 to that of the first plate) during which any proper motion so large as that indicated would easily declare itself. But comparison of these plates shows no sensible proper motion at all, so that the displacements shown in Table I. must be due to errors in Baxendell's chart. It affords an additional check on the Oxford results, if such be needed, that a comparison was made in the first instance with the Franklin-

Baxendell's Chart of S Vulpeculæ.

Adams chart of the region, and these displacements were first noticed then.

TABLE I.

Comparison of Baxendell's Chart of 1861 *with Oxford Plate No.* 1112, *centre* $19^h\ 48^m + 27°$, *exposed* 1897 *Sept.* 3.

Oxford.				Bax. Chart.			Bax. – Oxford.	
No.	*d.*	*x.*	*y.*	No.	X.	Y.	Δx.	Δy.
					mm.	mm.	″	″
...	20	(1·877)	(11·811)	7	−37·7	−22·9	−15	−21
53317	13	1·000	11·670	13	−21·8	−24·9	−12	− 9
53451	20	1·926	14·137	2	− 8·8	+10·8	− 6	+15
53409*	17	2·543	13·407	1	0·0	0·0	− 3	0
53320	11	2·941	11·701	16	+ 6·2	−23·7	+ 6	+ 6
53540	24	3·000	16·670	30	+ 6·7	+46·1	+ 6	+ 6
53321	9	3·026	11·304	14	+ 6·8	−30·2	− 3	−15
53453	12	3·050	14·528	32	+ 7·0	+15·8	− 3	+ 3
53541	23	3·354	16·306	28	+11·4	+41·3	+ 3	+12
53412	10	4·498	13·692	36	+27·8	+ 4·9	+ 6	+18
53368	10	4·648	12·903	15	+30·1	− 6·3	+ 6	+15
53323	8	4·947	11·154	18	+32·1	−28·2	−42	+69
53500*	23	4·971	15·067	21	+34·2	+23·6	0	+ 3
53501	18	4·992	15·524	24	+35·0	+29·8	+15	+ 3
53545	17	5·599	16·429	29	+43·5	+43·0	+15	+ 6
53324	12	6·198	11·420	38	+51·3	−27·5	− 9	+ 3
53370	12	6·562	12·434	37	+57·2	−18·3	+ 9	−75
...	5	(6·588)	(13·142)	22	+59·6	− 4·5	+51	−21
53293*	40	6·610	10·896	6	+57·0	−34·5	−12	+ 9
53327*	27	6·835	11·867	8	+60·4	−21·9	− 6	−15

It seems probable that Baxendell's measures were made with a position micrometer, and that there is an error of 60″ in the *distance* of star 18. The residuals −42″ and +69″ would then become 0 and +27″, which do not differ excessively from neighbouring ones. The error for No. 37 corresponds to about 3° in position angle.

The following stars of Baxendell's chart have been identified on the other Oxford plate :—

TABLE II.

Stars on Oxford Plate No. 2583, centre $19^h\ 40^m + 27°$, *exposed* 1907 *Sept.* 20.

Oxford.				Bax. Chart.			Bax. – Oxford.	
No.	*d.*	*x.*	*y.*	No.	X.	Y.	Δx.	Δy.
					mm.	mm.	″	″
...	5	20·654	13·288	4	−57·8	+ 1·5	+11	−11
...	5	20·745	13·338					
52088	13	21·427	13·171	5	−48·5	0·0	−10	+ 2
52090	14	22·873	13·619	3	−27·0	+ 6·0	+10	+ 1
52057	14	23·836	12·676	11	−14·0	− 8·5	+ 1	−21
52092	14	23·840	13·949	17	−13·5	+10·0	+ 5	−10
52093	23	24·193	13·928	2	− 8·5	+10·2	+ 5	0
52096*	22	24·819	13·217	1	0·0	0·0	+ 2	+ 1

Baxendell's No. 4 was not measured on the Oxford plate, but on search being made a pair of very faint stars was found and measured.

For these comparisons the chart measures in mm. were first multiplied by ·07 to convert them into units approximately comparable with Oxford measures; next the constants

for plate $19^h\ 48^m$, $x = 2·670$, $y = 13·380$
and for plate $19^h\ 40^m$, $x = 24·820$, $y = 13·220$

were added; and then approximate linear formulæ were found and applied to the differences.

This discussion is a digression, because Baxendell used only very few of the stars plotted. But it will serve to illustrate the general nature of his work on the charts of his variables.

TABLE III.

List of Comparison Stars used.

Bax. No.	A.S.V.		Deduced Mag.	H.P.	Adopted.
	No.	Grades.			
8	10	26, 22	7·64	7·91	85
9	29	61, 71	8·98	...	90
10	45	82, 94	9·58	...	96
2	52	92, 105	9·87	9·70	99

The deduced magnitudes in the fourth column were derived by plotting curves for the two sets of grades against the 17 H.P. stars given in the A S.V., then converting the grades into magnitudes and taking the mean. Two of the stars are, however, given directly in the H.P. as in the fifth column, and it will be seen Baxendell's 8 is sensibly fainter in H.P. than as given by the A.S.V. grades. Such indications as are afforded by Baxendell's

observations would make it fainter still, and accordingly the magnitude 8·5 has been adopted. For his star 2, on the other hand, Baxendell agrees more nearly with the A.S.V. But the material for comparison is scanty, as will be seen from Table IV.

TABLE IV.

Baxendell's Differences between his Comparison Stars.

Years.	90–85.	96–90.	99–96.	99–90.	99–85.	96–85.
1861–4	$5{\cdot}6_{15}$	$6{\cdot}2_{18}$	$3{\cdot}6_{20}$	$9{\cdot}0_{24}$	$14{\cdot}0_{17}$	$11{\cdot}5_{13}$
1865–6	$6{\cdot}0_{2}$	$5{\cdot}8_{20}$	$4{\cdot}9_{8}$	$11{\cdot}9_{7}$	$18{\cdot}3_{2}$	$12{\cdot}3_{2}$
1867–8	?$5{\cdot}0_{1}$	$6{\cdot}6_{11}$	...	$10{\cdot}0_{1}$	?$15{\cdot}0_{1}$	...
1878–80	$4{\cdot}3_{2}$	$8{\cdot}3_{4}$	...	...	...	...
1884–7	$5{\cdot}0_{1}$	$8{\cdot}7_{25}$	...	...	...	$13{\cdot}0_{1}$
Mean	5·5	7·0	4·0	9·7	14·5	11·7
No. of Obs.	21	78	28	32	20	16
Reduced Mean	4·5	6·1	3·4	8·0	12·0	9·7

The interval between the stars 9 and 10 (96–90) increases almost steadily.

The telescopes used were A 68 in the first series (to end of 1868) and E 80 in the later series, except that R (13) 81 was used 1864 Oct. 7 and 1868 July 27; R (7) 39, 1863 Aug. 15; and *a* (22) 39, 1863 Sept. 19. Also, on the first four dates of the whole series, the telescope and power are not given.

TABLE V.

Baxendell's Observations of S (2) Vulpeculæ.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1861	240+		
Nov. 27	1107	99−9, 90−1	90
1862			
Apr. 29	1260	85+4, 99−6, 90+6, 96−2	91
June 28	1320	85+6, 99−5, 90+2, 96−4	92
July 28	1350	85+4, 99−6	91
Sept. 20	1404	85+6, 99−4, 90+3, 96−2	93
1862	240+		
Oct. 29	1443	99−6, 85+3, 90−1	90
Nov. 24	1469	85+7, 99−1, 90+3, 96−1	94
1863			
Aug. 15	1733	85+5, 99−3	93
Sept. 5	1754	99−7, 90−2	90
7	1756	85+3, 90−2	88
19	1768	*90+3, *96−1, 85+7, 99−6	?93

* Baxendell writes, Sept. 20, "Qu. as to last night's comparisons with 9 and 10?" and on Sept. 19 he writes, "Does 2 change?"

TABLE V.—*Baxendell's Observations of S* (2) *Vulpeculæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1863	240+		
Sept. 20	1769	85+4, 90−3, 99−6	90
Oct. 9	1788	85+7, 99−4, 90+2, 96−3	93
22	1801	85+12, 99−4, 90+3, 96−2	95
30	1809	85+11, 99−3, 90+3, 96−2	95
Nov. 19	1829	85+5, 99−9, 90+1, 96−4	*91
Dec. 17	1857	90+4, 96−1, †85+8, 99−4	94
27	1867	85+8, 99−6, 90+3, 96−2	93
1864			
May 13	2005	85+8, 96+0, 99−5	94
July 4	2057	90+1, 96−3, 99−6	92
14	2067	90+3, 96−3, 99−6, 85+9	93
29	2082	90+3, 96−3, 99−6	93
Aug. 21	2105	90−1, 99−8	90
Sept. 9	2124	90+1, 96−3, 99−6	92
Oct. 7	2152	99−6	‡93
9	2154	90+4, 96−2, 99−6	94
17	2162	90+3, 96−3, 99−7	93
24	2169	90−1, 96−6, 99−7	90
1864	240+		
Oct. 31	2176	90−1, 99−9	90
Nov. 23	2199	90+0	90
Dec. 6	2212	96+0, 99−4	§96
21	2227	96−3	‖93
1865			
Apr. 15	2342	90+4, 96+0, 85+8, 99−5	94
29	2356	90+5, 96−1, 85+10, 99−6	94
May 15	2372	90+0, 96−4	91
20	2377	90+1, 96−4	92
June 9	2397	90+0	¶90
13	2401	90+3, 96−3, 99−7	93
17	2405	90+3, 96−2	94
July 1	2419	90+4, 96−1	94
14	2432	90+1, 96−3	92
Aug. 3	2452	90+1, 96−3	92
25	2474	90+3, 96−2	94
Sept. 7	2487	96−1, 99−3	96
28	2508	90+1, 96−4	92
Nov. 1	2542	90+3, 96−3	93
1866			
Apr. 23	2715	99−9, 90+2, 96−4	91
May 17	2739	96+0, 99−3	96
24	2746	90+4, 96−2, 99−5	94
June 25	2778	90+0, 99−10	90
July 11	2794	90+2, 96−3	92
22	2805	90+0	90
Aug. 27	2841	90−1	89

* "☽ and v. hazy."
† Baxendell writes, "Does 8 vary?"
‡ This figure is followed by a double note of admiration by Baxendell in his list of deduced magnitudes.
§ "☽ and hazy."
‖ "v. low and clouds troublesome."
¶ This is followed by a double note of admiration by Baxendell in his list of deduced magnitudes.

TABLE V.—*Baxendell's Observations of S (2) Vulpeculæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1866	240+		
Sept. 17	2862	90−1, 96−5	90
Oct. 6	2881	90+3, 96−3	93
16	2891	96+0	96
Nov. 3	2909	90−1, 96−6, 99−9 [? *85+5]	90
9	2915	90−1	89
19	2925	90−1	89
25	2931	90−0	90
30	2936	90+0	90
Dec. 16	2952	90+3, 96−2	94
30	2966	90+3, 96−3	93
1867			
Jan. 11	2978	90−1 [?†85+3], 99−9	89 (or 90)
June 5	3123	90+0	90
Sept. 5	3215	90+2, 96−3	92
10	3220	90+3, 96−3	93
16	3226	90+3, 96−3	93
24	3234	90+3, 96−3	93
Oct. 23	3263	90−1	89
Nov. 1	3272	90−1	89
19	3290	90+3, 96−3	93
Dec. 12	3313	90+1	91
1868			
Apr. 11	3434	96+0	96
23	3446	90+3, 96−3	93
May 11	3464	90+0	90
20	3473	90+1	91
31	3484	96−1	95
June 7	3491	96−2	94
16	3500	96−2	94
19	3503	90+4, 96−2	94
28	3512	90+3, 96−3	93
July 11	3525	90+0	90
21	3535	90+0	90
1868	240+		
July 25	3539	90+0	90
27	3541	90+3, 96−3	93
Sept. 6	3582	90+3, 96−3	93
Oct. 17	3623	90+3, 96−3	93
Nov. 6	3643	90−0	90
8	3645	90−0	90
15	3652	90−1	89
1878			
June 29	7165	90+5, 96−4	94
1880			
Sept. 6	7965	90+0, 85+2	88
7	7966	85+4, 90−1	89
Oct. 10	7999	90+4, 96−3	94
20	8009	90+2, 96−4	92
Nov. 3	8023	90+3, 96−3	93
1884			
Aug. 24	9413	90+3, 96−3	93
25	9414	90+3, 96−3	93
Sept. 2	9422	90+3, 96−3	93
11	9431	90+4, 96−3	94
17	9437	90+6, 96+0	96
26	9446	90+6, 96−2	95
Oct. 1	9451	90+5, 96−4	94
5	9455	90−2	88
12	9462	90−1	89
Nov. 9	9490	90+5, 96−2	94
13	9494	96−2	94
20	9501	96+2	98
27	9508	96+0	96
Dec. 4	9515	90+6, 96−3	94
20	9531	90+1, 96−6, 85+5	90
24	9535	90+3, 96−7	91

* The number 5 is given in the observation, but it seems almost certainly a slip for 8.

† The number is omitted in the entry, but 8 is supplied conjecturally.

TABLE V.—*Baxendell's Observations of S* (2) *Vulpeculæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1885	240+		
Jan. 8	9550	96−2	94
11	9553	90+5, 96−3	94
May 11	9673	90+1, 96−5	91
20	9682	90+3, 96−3	93
June 1	9694	90+7, 96−1	96
9	9702	90+7, 96−1	96
July 4	9727	90+2, 96−5	92
29	9752	90+4, 96−3	94
Aug. 14	9768	96−1	95
19	9773	96−0	96
Sept. 5	9790	90+1	91
25	9810	90+1	91
Oct. 29	9844	96+0	96
Nov. 8	9854	96−1	95
15	9861	90+3, 96−5	92
19	9865	90−2	88
Dec. 1	9877	90+0	90
26	9902	90+4, 96−3	94
1886			
Jan. 6	9913	96+1	97
11	9918	96+1	97
	241+		
June 14	0072	90+0	90
Sept. 18	0168	90+1, 96−6	90
Oct. 23	0203	90−2	88
Nov. 2	0213	90−1	89
4	0215	90+0	90
17	0228	90+2, 96−6	91
Dec. 4	0245	90+5, 96−3	94
20	0261	96−2	94
29	0270	90+2, 96−7	90
1887			
Jan. 12	0284	90+3, 96−4	92

The observations were arranged in the usual manner, with an assumed period of 67·5 days, Chandler's formula being 2402239·0 +67·5 E; but they were found to be too scanty to give good results by the usual methods. They were therefore plotted on squared paper, and it was found that a period of 67·5 days fitted them very well on the whole, both for maximum and minimum. It is never possible to fix these epochs *exactly* from the observations; but when the dates in the following table are compared with the observations, it is seen that in very few cases is there any serious discrepancy. The minima of E = −1, 113, 116, and 118 seem to fall later than the mean date, and those of E = 18 and −6 earlier; and the maxima of E = 85 and 107 seem to fall earlier, and that of E = 15 later; but in most of these cases the discrepancy rests upon a single observation—sometimes even upon *half* an observation only!

In the rest of the epochs the mean date, while not always quite the most "probable" one, is not far from being so, and is reasonably consistent with the observations. Chandler's epochs for maximum are 5 days earlier throughout, which, as a rule, makes no appreciable difference to the agreement; but the dates here given seem to be slightly better in more instances than those in which they are slightly worse. The minimum is put at 36 days after the maximum, but there are some indications of a progressive increase in the distance from maximum to minimum, as though the period from minimum to minimum were a little longer than from maximum to maximum.

TABLE VI.

Mean Epochs observed by Baxendell of S Vulpeculæ.

E.	Max. 240+	Min.
−17	1096·5	...
−15	...	1267·5
−14	(1299·0)	1335·0
−13	(1366·5)	1402·5
−12	(1434·0)	1470·0
− 7	1771·5	1807·5
− 6	1839·0	1875·0
− 4	...	2010·0
− 3	2041·5	2077·5
− 2	...	2145·0
− 1	2176·5	2212·5
0	(2244·0)	...
+ 1	...	2347·5
+ 2	2379·0	2415·0
+ 3	2446·5	2482·5
+ 7	2716·5	2752·5
+ 8	2784·0	(2820·0)
+ 9	2851·5	(2887·5)
+10	2919·0	2955·0
+11	2986·5	...
+ 13	3121·5	...
+ 14	...	3225·0
+ 15	3256·5	3292·5
+ 16	(3324·0)	...
+ 17	...	(3427·5)
+ 18	(3459·0)	3495·0
+ 19	3526·5	3562·5
+ 20	(3594·0)	3630·0
+ 21	(3661·5)	...
+ 85	7981·5	8017·5
+106	...	9435·0
+107	9466·5	9502·5
+108	9534·0	(9570·0)
+110	9669·0	...
+111	9736·5	9772·5
+112	9804·0	9840·0
+113	9871·5	9907·5
+116	10074·0	...
+117	...	10177·5
+118	10209·0	10245·0
+119	10276·5	...

There is in a good many cases some suggestion of a secondary maximum and minimum, not (as with R Sagittæ) about 180° from the primary ones, but at about 8 or 10 days before or after these. But this appearance may be illusory, and be really due to considerable flatness in maximum and minimum, combined with variable atmospheric conditions.

On further consideration, and at the suggestion of Mr. J. Baxendell, junior, it has been decided to add to the 23 stars already discussed, a few others, beginning with S Aquilæ, of which Baxendell took many observations. After the first four years of observation he called it T instead of S.

No. 24. *S Aquilæ* (*Chandler* 6726).

$20^h\ 4^m\ 57^s$ $+15^\circ\ 11'5$ (1855·0)
20 7 1 +15 19·4 (1900·0)

The Comparison Stars.—Baxendell made two charts and one small sketch for this variable, and the identifications do not present

much difficulty. The *b* of the first chart was clearly shown to be variable. Each *b* was very seldom compared with the variable.

TABLE VII.

Comparison Stars.

First Chart.	Second Chart.	Sketch, 1863 Aug. 24.	A.S.V. No.	A.S.V. Grades.	H.P.	Adopted Mags.	Bax.'s Est.
a	*a*	*a*	27	90, 78	9·36	94	9·2 to 9·7
m	*b*	...	30	96, 86	9·64	96	9·4
b	...	...	33	(98), 87	9·34	99	9·9 ?
c	*c*	...	41	111, 93	...	102	10·1
d	*d*	*d*	50	121, 101	10·69	106	10·4
e	*e*	*e*	53	129, 109	...	112	11·0
...	*f*	*f*	54	146, 123	...	120	12·0
r	...	...	49	121, 101	10·60	...	...
...	...	*g*	...	...	...	123	...
...	...	*h*	...	...	...	...	...
(A)	...	...	?13	5, 43	8·49	85	8·0 ?

The *g* and *h* on the sketch are not given in Hagen's chart.

The *b* of the first chart was discarded after 1863 Oct. 11, from which date the name *b* was transferred to the star previously called *m*; but the latter star was also discarded almost entirely after the 28th of the same month until 1878. A is not named on any of the charts, and its identity is not certain.

TABLE VIII.

Baxendell's Differences between his Comparison Stars.

Years.	102–96.	102–99.	106–102.	112–106.	120–112.	102–94.	112–102.	94–85.
1863–4	$7\cdot0_{1}$	$0\cdot5_{9}$	$2\cdot3_{14}$	$6\cdot7_{27}$	$5\cdot4_{12}$	$9\cdot0_{13}$	$7\cdot0_{4}$	$12\cdot1_{9}$
1865–7	...	...	$2\cdot6_{13}$	$8\cdot1_{24}$	$12\cdot0_{1}$	$6\cdot5_{26}$	$8\cdot0_{2}$	$13\cdot1_{4}$
1868–9	$1\cdot0_{1}$	...	$2\cdot5_{4}$	$6\cdot2_{16}$	...	$6\cdot7_{3}$	...	...
1877–9	$6\cdot2_{8}$	...	$2\cdot6_{15}$	$7\cdot8_{20}$	$13\cdot0_{11}$	$7\cdot0_{21}$	$9\cdot0_{3}$	...
1880–3	...	...	$4\cdot0_{16}$	$7\cdot1_{14}$	$14\cdot5_{5}$	$7\cdot7_{41}$	...	...
1884–6	...	...	$4\cdot8_{28}$	$8\cdot0_{30}$	...	$7\cdot3_{17}$	$9\cdot5_{2}$	...
Mean.	5·8	0·5	3·5	7·4	10·1	7·4	8·2	12·4
Reduced Mean.	5·4	0·4	3·1	6·4	8·4	6·4	7·1	10·8
No. of Obs.	10	9	90	131	29	121	11	13

Besides these, 5 observations of 96–94 in the years 1863–4 give a mean difference 2·4, or 2·2 when reduced; and 4 observa-

tions of 99–94 in the same years give a mean difference 6·4, or 5·4 when reduced.

The instruments used were A 68 until 1869 Oct. 26, and E 80 afterwards, with the following exceptions:—R (7) 130 was used 1863 July 28 to Aug. 13; C 39, 1863 Aug. 15 and Sept. 19, 1866 Aug. 3; R (13) 81, 1868 June 18; A 39, 1877 Sept. 4; E 110, 1877 Dec. 31. The intervals obtained between some of the comparison stars vary very much, and Baxendell considered *d* to be variable, but he continued to make use of it.

TABLE IX.

Baxendell's Observations of S Aquilæ.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1859	240+		
Aug. 19	0276	(About) 94+0	94 ?
1860			
June 1	0563	Invisible.	
1863			
June 25	1682	94+0	94
July 28	1715	94+11, <99	*105
29	1716	94+12, 102+3, 106+3	*107
31	1718	102+4, 106+3, 112+0	109D
Aug. 1	1719	112+0, 102+6, 106+5	110
4	1722	112+0, 106+5	112
13	1731	112+2	114
15	1733	112+1	113
20	1738	112+2, 120+1	118
24	1742	112+3, 120−1, 123−3	118
29	1747	112+3	115
Sept. 3	1752	120−2	118
5	1754	112+1, 120−4	114
7	1756	112+0, 120−5	114
10	1759	112+0	112
19	1768	112−3, 106+2	108
20	1769	106+3, 112−3	109
22	1771	106+1, 112−4	108
Oct. 9	1788	94+4, 106−2	101
Oct. 11	1790	94+6, 102−2	100
,,	,,	94+4, 102−2	99
13	1792	94+4, 102−2	99
24	1803	94−3	91
28	1807	94−2	92
29	1808	94−2	92
30	1809	94−1	93
Nov. 1	1811	94−2	92
2	1812	94−2	92
6	1816	94−1	93
9	1819	94+0	94
10	1820	94+0	94
11	1821	94+0	94
19	1829	102+1, 94+8	102
28	1838	106+3, 112−3	109
29	1839	106+3, 112−3	109
Dec. 5	1845	106+3, 112−3	109
17	1857	106+2, 112−3	108
22	1862	106+1, 112−4	108
27	1867	102+1, 112−5, 106−1	105
1864			
Jan. 1	1872	102+3, 112−3, 106+2	107
May 16	2008	94+4, 102−2	99
26	2018	102+2, 106−1	104
June 6	2029	106+3, 112−3	109
16	2039	106+6, 112−1	112

* "Strong D and hazy."

TABLE IX.—*Baxendell's Observations of S Aquilæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1864	240+		
July 4	2057	112+1	113
8	2061	112−1	111
13	2066	106+4, 112−2	110
16	2069	106+3, 112−3	109
19	2072	106+3, 112−3	109
26	2079	106+3, 112−3	109
29	2082	106+3, 112−3	109
Aug. 21	2105	85+2, 94−7	87
23	2107	85+3, 94−7	88
26	2110	85+3, 94−7	88
31	2115	85+4, 94−7	88
Sept. 5	2120	85+3, 94−6	88
9	2124	85+5, 94−6	89
11	2126	85+6, 94−5	90
14	2129	85+6, 94−6	90
19	2134	85+7, 94−5	90
24	2139	94−1	93
26	2141	94+0	94
Oct. 1	2146	94+0	94
3	2148	94+1	95
5	2150	94+3	97
8	2153	94+6, 102−1	100
17	2162	106+4, 112−3	110
24	2169	106+6, 112−3	110
31	2176	112+0	112
Nov. 5	2181	112+0	112
8	2184	112+0	112
23	2199	112+4	116
1865			
May 18	2375	112+2	114
20	2377	112+1	113
24	2381	112+2	114
June 8	2396	106+1, 112−4	108
9	2397	106+0	106
11	2399	106+0	106
13	2401	106+0	106
June 17	2405	102+1, 106−1	104
20	2408	102+2, 106−1	104
21	2409	102+1, 106−2	104
28	2416	102+0	102
July 1	2419	102+0	102
13	2431	102+1, 106−2	104
14	2432	102+1, 106−2	104
21	2439	102+2, 106+1	106
Aug. 3	2452	106+6, 112−1	112
12	2461	112+0	112
25	2474	112+2	114
31	2480	112+4, 120−6	115
Sept. 7	2487	112+2	114
23	2503	112−1	111
27	2507	106+4, 112−3	110
Oct. 2	2512	106+4, 112−3	110
7	2517	106+2, 112−5	108
10	2520	106+4, 112−3	110
19	2529	106+4, 112−3	110
29	2539	106+2, 112−6	107
Nov. 1	2542	106+0	*106
5	2546	102+0, 106−2	103
7	2548	94+4, 102−2	99
12	2553	94+4, 102−2	99
15	2556	94+3, 102−3	98
26	2567	94+1, 102−3	97
Dec. 22	2593	102+2, 106+0 (about)	†105?
31	2602	102+3, 112−3	‡107
1866			
May 17	2739	106+2, 112−6	107
20	2742	106+2, 112−4	108
22	2744	106+2, 112−4	108
24	2746	106+2, 112−4	108
June 8	2761	106+4, 112−2	110
25	2778	102+1, 106−2	104

* "☽ and hazy." † "Sky thick." ‡ "☽ and cloud."

TABLE IX.—*Baxendell's Observations of S Aquilæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1866	240+		
July 11	2794	94+0, 102−2	97
16	2799	94+2, 102−3	98
20	2803	94+3, 102−3	98
22	2805	94+3, 102−3	98
30	2813	94+3, 102−3	98
31	2814	94+4, 102−2	99
Aug. 3	2817	94+6, 102−1	100
20	2834	94+1, 102−3	*97
27	2841	94−3, 85+8	92
31	2845	85+6, 94−4	90
Sept. 8	2853	85+6, 94−5	90
15	2860	85+10, 94−2	94
17	2862	94+0	94
24	2869	94+4, 102−3	98
Oct. 1	2876	94+3, 102−3	98
6	2881	94+3, 102−2	98
9	2884	94+5, 102−1	100
10	2885	94+0, 102−5	96
14	2889	94+8, 102+2	103
16	2891	102+3, 106+1	106
19	2894	106+2, 112−8	106
28	2903	106+3, 112−4	108
Nov. 3	2909	106+6, 112−3	110
6	2912	112−3	109
9	2915	112−1	111
19	2925	112−1	111
25	2931	106+6, 112−3	110
30	2936	106+3, 112−5	108
Dec. 16	2952	106+1, 112−7	106
30	2966	106+0, 112−6	106
1867			
Jan. 11	†2978	94+9, 102−3	101
		?94−3, 102−9	92
May 21	3108	102+2	104
June 29	3147	94+4, 102−3	98
July 24	3172	102+1, 106−1	104
July 27	3175	102+2, 106+0, 112−5	106
Aug. 1	3180	106+1, 112−5	107
6	3185	106+2, 112−3	108
23	3202	112+0	112
Sept. 2	3212	112+0	112
10	3220	112−1	111
16	3226	112−1	111
23	3233	112+0	112
24	3234	112−1	111
25	3235	112−1	111
27	3237	112−2	110
Oct. 3	3243	112−1	111
8	3248	112−1	111
17	3257	94+3, 102−3	98
23	3263	94−2	92
Nov. 1	3272	94−2	92
17	3288	94−4	90
19	3290	94−2	92
Dec. 6	3307	94+4, 102−3	98
12	3313	102+1, 106−2	104
18	3319	106+2, 112−4	108
1868			
May 20	3473	106+2, 112−3	108
25	3478	106+3, 112−2	110
June 2	3486	106+3, 112−3	109
4	3488	106+3, 112−2	110
7	3491	106+3, 112−3	109
12	3496	106+3, 112−2	110
15	3499	106+4, 112−2	110
18	3502	106+4, 112−1	110
19	3503	106+4, 112−1	110
25	3509	106+5, 112+0	112
26	3510	106+5, 112−1	111
28	3512	106+4, 112−2	110

* "Clouds—doubtful."

† The two lines are both Baxendell's.

TABLE IX.—*Baxendell's Observations of S Aquilæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1868	240+		
June 29	3513	112−2	110
30	3514	112−2	110
July 6	3520	106+3, 112−3	109
10	3524	102+1, 106−2	104
11	3525	102+0	102
12	3526	102−1	101
13	3527	94+4, 102−2	99
21	3535	102+1, 106−2	104
23	3537	102+0	102
25	3539	102+0	102
29	3543	102−1	101
Aug. 3	3548	96−2, 102−3	96
Sept. 4	3580	94−2	92
5	3581	94−1	93
6	3582	94−1	93
11	3587	94+0	94
Oct. 17	3623	106+1	107
19	3625	106+0, 112−5	106
27	3633	102+0, 106−2	103
Nov. 6	3643	About 102+0	* 102 ?
8	3645	102+1, 106−1	104
15	3652	106+3, 112−3	109
1869			
June 8	3857	94−6	88
Oct. 26	3997	106+2, 112−3	108
Nov. 28	4030	94+2, 102−4	97
1877			
Sept. 4	6867	94+3	†97
Nov. 25	6949	94−1	93
Dec. 2	6956	94−1	93
13	6967	94+2	96
14	6968	94+3, 102−3	98
17	6971	94+2	96
18	6972	94+2	96
Dec. 23	6977	94+4	‡98
31	6985	106+3, 112−2	110
1878			
June 25	7161	106+4, 112−1	110
28	7164	112−1	111
July 7	7173	112−1	111
12	7178	112+0	112
19	7185	106+2	108
20	7186	106+3, 112−6	108
21	7187	106−1, 112−6	§106
27	7193	102+4, 106−2	105
Aug. 4	7201	102+3, 106−1	105
10	7207	106−3, 102−1	102
18	7215	96+3, 102−1	100
21	7218	102−1	101
25	7222	96+5, 102−1	101
29	7226	96+4, 102−1	100
Sept. 2	7230	94+3, 102−2	98
5	7233	96+4, 102−2	100
11	7239	96+0, 102−3	98
15	7243	94−3, 102−6	94
24	7252	94+5, 102+0	100
Oct. 11	7269	102+1, 94+9	103
17	7275	106+2, ‖102−1	104
26	7284	106+3, 112−2	110
Nov. 1	7290	106+6, 112−2	111
4	7293	106+4, 112−3	110
8	7297	106+6, 112−2	111
17	7306	112+0	112
19	7308	112+0	112
28	7317	112−1	111
Dec. 5	7324	112−2	110
7	7326	112−2	110
8	7327	106+6, 112−2	111

* "Cloud."
† "Cloud."
‡ "Hasty observation."
§ "Does *d* change?"
‖ Baxendell writes, "*d* is certainly brighter than *c*."

TABLE IX.—*Baxendell's Observations of S Aquilæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1878	240+		
Dec. 23	7342	102+4, 112−5, 106+3	107
30	7349	106+3	109
1879			
Jan. 13	7363	102+1, 106−3	103
15	7365	102−1	101
19	7369	94+3, 102−1, 106−6	99
22	7372	94+7, 102+1	102
Aug. 1	7563	112−2	110
8	7570	106+0, 112−6	106
11	7573	96+3, *102−1?	100?
14	7576	102+3, 106+0	106
17	7579	106+0, 102+3	106
21	7583	106+3, 112−5, 102+3	107
23	7585	112−2	110
26	7588	106+3	109
29	7591	106+4, 112−3	110
31	7593	106+6, 112−1	112
Sept. 2	7595	106+7, 112+0	112
7	7600	106+8, 120−6, 112+3	114
9	7602	112+2, 120−8	113
12	7605	112+2, 120−7	114
13	7606	112+1, 120−9	112
19	7612	112+3, 120−9	113
20	7613	112+3	115
21	7614	112+2	114
23	7616	112+3	115
25	7618	112+4, 120−6	115)
26	7619	112+5	117))
28	7621	112+7	119)
29	7622	112+6	118))
Oct. 2	7625	112+6, 120−4	117
4	7627	112+6, 120−6	116
5	7628	112+6, 120−6	116
6	7629	112+6, 120−6	116
14	7637	112+1, †120−12	110?
15	7638	112+0	112
16	7639	112−1	111
17	7640	112+0	112
25	7648	106−2, 112−10	103
26	7649	102+0, 106−4	102
28	7651	>112	
Nov. 1	7655	96+3, 102−3	99
2	7656	96+4, 102−2	100
5	7659	94+1, 102−3	97
7	7661	94+2, 102−3	98
10	7664	94+1, 102−3	97
12	7666	94+1, 102−3	97
13	7667	94+1, 102−3	97
22	7676	94−1	93
23	7677	94+1	95
24	7678	94+2, 102−3	98
25	7679	94+0	94
27	7681	94+1	95
29	7683	94+3, 102−4	98
30	7684	94+3, 102−3	98
Dec. 3	7687	94+4, 102−3	98
6	7690	94+4, 102−5	98
22	7706	94+6, 102−1	100
23	7707	94+6, 102−1	100
24	7708	94+7, 102−2	100
26	7710	94+8, 102+0	102
28	7712	102+1	103
1880			
Jan. 2	7717	102+2, 106−2	104
Mar. 17	7792	102+1, 106−3	103
Apr. 17	7823	94+0	94
24	7830	94+1, 102−4	96
29	7835	94+3, 102−2	98

* The query is Baxendell's.

† "*f* seems less than usual."

TABLE IX.—*Baxendell's Observations of S Aquilæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1880	240+		
Apr. 30	7836	94+3, 102−2	98
May 5	7841	94+4, 102−1	100
7	7843	94+7, 102−1	101
12	7848	102+3, 106−1	105
13	7849	106+1, 112−4	108
15	7851	102+2, 106−2	104
27	7863	106+5, 112−2	110
June 4	7871	112−1	111
7	7874	112+0	112
17	7884	112+1	113
26	7893	112+4, 120−8	114
July 3	7900	112+4 (or more)	*116!
4	7901	112+4, 120−8	114
9	7906	112+6, 120−6	116
20	7917	112+9, 120−4	118
30	7927	112+0	112
31	7928	112+0	112
Aug. 6	7934	102+0, 106−1	104
9	7937	106−1, 112−4	106
11	7939	102+0, 106−3	102
14	7942	102−1	101
16	7944	94+4, 102−3	98
17	7945	94+2, 102−6	96
22	7950	94+3, 102−3	98
24	7952	94+5, 102−3	99
27	7955	94+6, 102−1	100
31	7959	94+4, 102−3	98
Sept. 2	7961	94+3, 102−5	97
3	7962	94+3, 102−5	97
4	7963	94+3, 102−3	98
7	7966	94+2, 102−4	97
8	7967	94+2, 102−4	97
13	7972	94+3, 102−3	98
16	7975	94+2, 102−4	97
18	7977	94+2, 102−6	96
25	7984	94+0, 102−7	94
Sept. 26	7985	94+0, 102−7	94
Oct. 2	7991	94+1, 102−7	95
3	7992	94+1, 102−5	96
9	7998	94+3, 102−6	96
17	8006	94+6, 102−2	100
20	8009	94+6, 102−1	100
23	8012	94+7, 102−1	101
29	8018	102+2, 106−2	104
31	8020	102−1, 106−4	102
Nov. 3	8023	102+3, 106−2	104
8	8028	106+2, 112−6	107
19	8039	106+3, 112−3	109
20	8040	106+4, 112−3	110
26	8046	†106+6, 112−2	111
Dec. 1	8051	106+5, 112−1	111
4	8054	112+1	113
18	8068	112−3	109
24	8074	106+3, 112−3	109
1881			
Jan. 12	8093	94+0	94
May 4	8205	106+5, 112−3	110
7	8208	112−1	111
9	8210	112−3	109
12	8213	106+1, 112−4	108
16	8217	106−1, 112−7	105
20	8221	102+0	102
21	8222	102+0	102
22	8223	102−1	101
23	8224	102−2	100
29	8230	94+0	94
30	8231	94+0	94
June 8	8240	94+1	95
12	8244	94+2	96
18	8250	94+7, 102−1	101
27	8259	94+3, 102−3	98
July 8	8270	94+0	94

* "Cloud."

† "$d>c$, ∴ one var."

TABLE IX.—*Baxendell's Observations of S Aquilæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1881	240+		
July 11	8273	94−1	93
14	8276	94+0	94
17	8279	94+0	94
19	8281	94+0	94
27	8289	94+0	94
Aug. 1	8294	94+1	95
5	8298	94+1	95
6	8299	94+1	95
24	8317	102+3, 106−2	104
31	8324	112+0	112
Sept. 1	8325	112+0	112
14	8338	112+2	114
19	8343	112+1	113
25	8349	112+1	113
Oct. 1	8355	112−1	111
2	8356	112−2	110
15	8369	112−2	110
16	8370	112−3	109
26	8380	102+3, 106+1	106
29	8383	106+0, 112−4	107
Nov. 5	8390	102−1	101
14	8399	9+43, *102−8	96?
		?(or 112−8) or 100	
17	8402	94+3, 102−3	98
20	8405	94+1, 102−3	97
22	8407	94+1, 102−4	96
28	8413	94−1	93
Dec. 1	8416	94+0	94
10	8425	94−1	93
19	8434	94+0	94
1882			
May 12	8578	94+0	94
14	8580	94+0	94
16	8582	94+2	96
18	8584	94+3, 102−2	98
19	8585	94+4, 102−1	100
20	8586	94+4, 102−2	99
May 25	8591	102+3, 106+0	106
28	8594	102+6, 106+0	107
29	8595	102+6, 106−1	106
July 9	8636	112+5, 120−6	116
24	8651	112+2	114
26	8653	112+1	113
29	8656	112−1	111
Aug. 19	8677	94+6, 102−3	100
Sept. 4	8693	94+3, 102−2	98
9	8698	94+4, 102−3	98
11	8700	94+3, 102−2	98
13	8702	94+2, 102−4	97
15	8704	94+0	94
Oct. 5	8724	94+3, 102−3	98
28	8747	112+0	112
1883			
July 28	9020	102+4, 106−1	106
Aug. 25	9048	106+3, 112−1	110
Oct. 28	9112	102+0	102
1884			
May 18	9315	102+2, 106−3	104
24	9321	102+3, 106−3	104
26	9323	102+2, 106−3	104
27	9324	102+4, 106−1	106
30	9327	102+3, 106−3	104
June 11	9339	106−1	105
20	9348	102+3, 106+0	106
25	9353	102+0, 106−3	102
July 2	9360	102+0, 106−3	102
6	9364	102−2	100
12	9370	94+4, 102−2	99
16	9374	102+0, 106−3	102
25	9383	94+4, 102+0	100
27	9385	102+0, 106−3, 94+5	101
Aug. 1	9390	102+0, 106−4	102

* The letter is *c*, but Baxendell adds "?*e*."

TABLE IX.—*Baxendell's Observations of S Aquilæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1884	240+		
Aug. 4	9393	102+1, 106−4	102
6	9395	102+1	103
11	9400	102+1, 106−4	102
14	9403	102+3, 106−2	104
15	9404	102+3, 106−2	104
16	9405	102+3, 106+2	*106 ?
18	9407	102+2, 106−2	104
19	9408	102+4, 106+1	106
23	9412	102+6, 106+1	108
25	9414	102+7, 106+3	109
Sept. 1	9421	102+5, 106+3	108
4	9424	106+3, 112−3	†109
11	9431	112+0	112
16	9436	112−1	111
17	9437	112−2	110
19	9439	106+3, 112−3	109
20	9440	106+3, 112−3	109
24	9444	102−1, 112−8, 106−3	‡103
Oct. 4	9454	94+1, 102−6	96
5	9455	94+3, 102−3	98
9	9459	94+3, 102−4	98
12	9462	94+0, 102−7	94
Nov. 7	9488	94+1	95
13	9494	94−2	92
17	9498	94−2	92
19	9500	94−2	92
21	9502	94−2	92
27	9508	94−3	‡91
28	9509	94−3	91
29	9510	94−2	92
Dec. 3	9514	94+2	96
4	9515	94+1	95
6	9517	94+3	97
8	9519	94+4	98
9	9520	94+5, 102−1	100
14	9525	106+4, 112−3, 94+9	107
15	9526	106+3, 112−3	109
Dec. 20	9531	106+6, 112−3	110
21	9532	106+6, 112−2	111
24	9535	112−2	110
1885			
Jan. 5	9547	106+2, 112−4	108
8	9550	106+4, 112−4	109
11	9553	112−2	110
14	9556	106+3, 112−5	108
Apr. 29	9661	106+0	106
May 7	9669	106+2, 112−3	108
11	9673	106+0, 112−6	106
20	9682	106+6, 112−2	111
June 1	9694	106+5, 112−2	110
9	9702	106+5, 112−1	111
12	9705	106+6, 112+0	112
July 4	9727	102+1, 106−5	102
9	9732	102−1	101
25	9748	94+2	96
28	9751	94+0	94
Aug. 1	9755	94+1	95
3	9757	94+2	96
14	9768	94+1	95
16	9770	94+1	95
18	9772	94+0	94
19	9773	94+0	94
20	9774	94+1	95
22	9776	94+1	95
28	9782	94+1	95
Sept. 3	9788	94+3	97
5	9790	94+3, 102−3	98
6	9791	94+5, 102−2	100
8	9793	94+7, 102+0	102
9	9794	94+6, 102+0	101
12	9797	102+1, 94+8	102
14	9799	106+0	106
18	9803	112−2	110
25	9810	112+1	113
26	9811	112+1	113

* "*d* evidently changes."
† "Clouds and ☽."
‡ "Observation repeated."

TABLE IX.—*Baxendell's Observations of S Aquilæ*—continued.

Date.	J.D.	Corrected Comparisons.	Deduced Mag.
1885	240+		
Sept. 27	9812	112+1	113
Oct. 29	9844	112−3	109
Nov. 8	9854	106+0, 112−4	107
15	9861	106+0	106
19	9865	106+2, 112−6	107
28	9874	106+0	106
Dec. 1	9877	106+0	106
26	9902	94+2, 102−5	96
1886			
Jan. 5	9912	94+4, 102−2	99
11	9918	94+4, 102−2	99
	241+		
May 21	0048	94+1	95
29	0056	94+0	94
June 5	0063	94+1	95
14	0072	94+2	96
26	0084	106+0	106
30	0088	106+6, 112+0	112
Sept. 14	0164	102+2, 106−2	104
17	0167	102+0	102
Oct. 23	0203	94+4, 102−2	99
Oct. 25	0205	94+4, 102+0	100
30	0210	102+2, 106−2	104
Nov. 2	0213	102+3, 106−3	104
4	0215	102+5, 106−2	106
7	0218	106+2	108
9	0220	106+3, 112−5	108
10	0221	106+3, 112−4	108
13	0224	106+4, 112−2	110
15	0226	106+5, 112−2	110
16	0227	106+6	112
17	0228	106+7, 112−2	112
18	0229	106+6, 112−3	110
30	0241	106+7, 112−1	112
Dec. 4	0245	106+7, 112−1	112
16	0257	102+6, 112−3, 106+4	109
20	0261	112−4	108
25	0266	106+1, 112−5	107
29	0270	106−1	105
1887			
Jan. 12	0284	102+2	104
June 10	0433	102−1	101

Later Observations of S Aquilæ by J. Baxendell, junior.

Date.	J.D.	Deduced Mag.
1887		
May 16	0408	111
June 7	0430	103½
10	0433	101
20	0443	94
24	0447	93
July 14	0467	90½
19	0472	90½
31	0484	94
Aug. 18	0502	108½
21	0505	110
Sept. 11	0526	111
1888		
May 11	0769	94
30	0788	94
June 18	0807	93½
July 26	0845	101
Aug. 2	0852	104
14	0864	100
27	0877	100½
Sept. 9	0890	100
17	0898	98
26	0907	99
Oct. 1	0912	98
15	0926	97
23	0934	*95
Nov. 14	0956	106½
1889		
May 20	1143	109½
25	1148	104
31	1154	98
June 16	1170	95
July 7	1191	95
27	1211	99
Aug. 15	1230	†112
Sept. 1	1247	111
30	1276	114
1890		
Sept. 10	1621	105

* Uncertain; fog and cloud. † "Hazy."

At first sight these observations present an appearance of great irregularity; but a closer inspection to a great extent removes this impression, and they have been tabulated in the usual manner with an assumed period of 147 days. The results, as far as Period LII., are as follows:—

	(1.)	(2.)	(3.)	(4.)	(5.)	(6.)	(7.)	(8.)	(9.)	(10.)	(11.)	(12
A (I. to XVI.) . .	97	101	104	108	110	112	108	106·5	103·5	100	97	94
B (XXXVI. to XLVIII.)	102	107	111	112	113	112	105	100	99	96	96	98
Weighted Mean (B shifted). .	98	102	105·5	109	111	112	110	106	102	99	96	95

There is an evident retrograde movement of about one "portion" ($12\frac{1}{4}$ days) between A and B, so the mean light curve has been obtained by shifting the B figures one place forward. This gives a good, smooth curve with minimum at 67 days and maximum at 136 days, after the arbitrary starting-point; but, before comparing the observations with it in the usual way, a table was drawn up to ascertain how far an amended regular period of 146·6 days would really fit the observed epochs of maximum and minimum, taking for these the widest reasonable limits suggested by the observations. The object of this was to ascertain whether the apparent anomalies were sufficiently numerous and irregular to make the work of obtaining the "corrections to epoch" not worth while. The result (given in Table X.) shows that, with a few startling exceptions, a regular period of 146·6 days is not far from accordance with the observations, especially when we take into account that the "limits" might sometimes be considerably extended by assuming a possible error or abnormal conditions in regard to one or two observations, and also when we notice how very much the magnitude *appears* to fluctuate very often near both maximum and minimum, and how much these vary in magnitude in different periods.

The individual observations have therefore been compared with the mean curve in all cases where they were available on both rise and fall, even when they were very few in number. They were so arranged as to bring the epoch of maximum as nearly as might be into the middle of each set, and *general* variations of brightness were allowed for. The results are given in the last two columns of Table X. In the cases of the last three epochs (from Mr. Baxendell, junior's, observations) the data are so scanty that the results must be considered very doubtful, and this is also the case with Periods X., XLVIII., LVI., and LVIII.

TABLE X.

Epochs of Minimum and Maximum of S Aquilæ.

Period.	Limits for Min.	Mean Min.	Least Diff.	Limits for Max.	Mean Max.	Least Diff.	Calc. Max.	C−M.
I.	1736–1750	1750	0	1800–1812	1819	−7	1807	−12
III.	2040–2059	2043	0	2100–2118	2112	0	2115	+ 3
V.	? –2385	2336	?+	2413–2424	2405	+8	2420	+15
VI.	2477–2483	2483	0	?2557–?2580	2552	?+5	2562	+10
VIII.	2754–2767	2776	−9	2843–2855	2845	0	2846	+ 1
IX.	2914–2928	2923	0	...	2992	...	...	...
X.	...	3069	...	...	3138	...	?3140	?+ 2
XI.	3195–3233	3216	0	3280–3289	3285	0	3285	0
XIII.	3506–3510	3509	0	?3569–3580	3578	0	3578	0
XV.	...	...	...	?3843–?3866	3871	?−5	...	...
XXXVI.	...	6881	...	?6925–6960	6950	0	...	...
XXXVIII.	7176–7180	7174	+2	7241–7246	7243	0	7241	− 2
XXXIX.	7302–7312	7321	−9	?7368–?7370	7390	?−20	...	...
XLI.	7620–7626	7614	+6	7672–7680	7683	− 3	7680	− 3
XLII.	...	7760	...	?7814–7826	7829	− 3	7824	− 5
XLIII.	7912–7920	7907	+5	?7945–7987	7976	?0	7981	+ 5
XLIV.	8053–8059	8054	0	...	8123	...	...	...
XLV.	? –8208	8200	?0	?8230–8290	8269	0	8271	+ 2
XLVI.	8333–8351	8347	0	8411–?8440	8416	0	...	...
XLVIII.	?8629–8643	8640	0	8704–8712	8709	0?	?8706	?− 3
LII.	...	9226	...	?9353–?9390	9296	?+57	9361	+65
LIII.	9428–9433	9373	+55	?9496–9509	9442	+54	9493	+51
LV.	?9680–9711	9666	+14	9750–9772	9735	+15	9765	+30
LVI.	9808–?9820	9813	?0	9890–9907	9882	+ 8	?9904	?+22
LVII.	...	9960	...	? –0067	0029	?+	...	...
LVIII.	...	0106	...	?0185–0203	0175	?+10	?0185	?+10
LIX.	0227–0250	0253	−3	...	0322	...	...	...
LX.	...	0400	...	0458–0476	0468	0	?0467	?− 1
LXIII.	?0849–?0857	0839	?+10	?0920–?0940	0908	?+12	?0913	?+ 5
LXV.	...	1132	...	1163–?1199	1201	?−2	?1183	?−18
LXVI.	?1262– ?	1279	?0	...	1348	...	...	...

In two cases the epochs obtained by calculation are outside the so-called "limits." Taking the last column as it stands, we see that the calculated epochs are in agreement with the "mean" ones, except for Periods I., V., VI., LII. to LVIII., and LXV.; but the last is quite doubtful.

These results suggest a *general* period of about 146·6 days,

with two (observed) fluctuations very similar in character. Each consists of a few periods abnormally long, followed by a few much shorter than the average, in both cases bringing the epoch back to its mean position. The variation is greater on the second occasion than on the first, and appears to extend over a longer time, though we may not have the beginning of the first one. There is, of course, ample time for one or two similar fluctuations during the gap between Periods XIII. and XXXVII., and there is, in fact, a distinct suggestion of a shortening of the period between XIII. and XV. made by the isolated observation of J.D. 2403857. The following discontinuity solution gives fairly good results, but it is possible that the fluctuations are really of a wave-like character.

Periods I. to V., 153·5 days; V. to VIII., 141 days; VIII. to XLVIII., 146·6 days; XLVIII. to LII., 161·5 days; LII. to LX., 139 days.

TABLE XI.

Period.	Epoch by Disc.	D−C.	Period.	Disc.	D−C.	Period.	Disc.	D−C.	Period.	Disc.	D−C.
I.	1807	0	X.	3137	?−3	XLII.	7828	+4	LIII.	9493	0
III.	2114	−1	XI.	3284	−1	XLIII.	7975	−6	LV.	9771	+6
V.	2421	+1	XIII.	3577	−1	XLV.	8268	−3	LVI.	9910	?+6
VI.	2562	0	XXXVIII.	7242	+1	XLVIII.	8708	?+2	LVIII.	0188	?+3
VIII.	2844	−2	XLI.	7682	+2	LII.	9354	−7	LX.	0466	?+1

The maximum magnitude varies, in different periods, from 8·7 to 9·9 or fainter; and the minimum from 11·0 (?) to 11·8.

Analysis of the mean light curve gives the expression

$$8{\cdot}08\cos(\theta - 138^\circ) + 0{\cdot}22\cos(2\theta - 90^\circ) + 0{\cdot}5\cos(3\theta - 90^\circ),$$

which becomes, when divided by the range and multiplied by 6, and when, further, the phase of the first harmonic is reduced to 180°, which the Rev. T. E. R. Phillips adopted as standard (*B.A.A. Journal*, October 1916, p. 9),

$$2{\cdot}85\cos(\theta - 180^\circ) + 0{\cdot}08\cos(2\theta - 174^\circ) + 0{\cdot}18\cos(3\theta - 216^\circ).$$

The figures for A (Periods I. to XVI.) and for B (XXXVI. to XLVIII.) give:

(A) $2{\cdot}52\cos(\theta - 180^\circ) + 0{\cdot}19\cos(2\theta - 230^\circ) + 0{\cdot}24\cos(3\theta - 174^\circ)$
(B) $2{\cdot}85\cos(\theta - 180^\circ) + 0{\cdot}14\cos(2\theta - 14^\circ) + 0{\cdot}29\cos(3\theta - 299^\circ)$.

Now, Mr. Phillips' classification depends on the relation between the phases ϕ_2 and ϕ_3 for the second and third harmonics. But it will be seen that the values of ϕ_2 for (A) and (B) differ by nearly 180°; and, when they are combined into the mean, not only is the coefficient 0·08 very small, but the phase is very uncertain. A change of a tenth of a magnitude in one reading might alter it by 90° or even more. Hence it seems unprofitable to attempt to assign the "group" of S Aquilæ from Baxendell's

observations alone, though the smallness of the coefficient of the second harmonic suggests Group I. rather than Group II.; but the point on Mr. Phillips's diagram falls near the junction of the two groups.

Notes on T Herculis and R Serpentis.

Since the publication of the sixth and seventh instalments of this series, Mr. C. L. Brook has very kindly allowed us to see the B.A.A. observations of these stars for J.D. 2418730 to 2420500 (about) in MS., and corrections to epoch for T Herculis have been calculated from these observations on the same plan which has been used throughout this series, so that they might be compared with the epochs given in *M.N.*, lxxvi. pp. 478–481 and 492. [There is a misprint on p. 478. The date for Period XIX., in Table VI., should be 3238, not 3288.] In the following Table the "assumed epochs," at 165 days' regular interval, are taken four days later than in the old Tables, as this suits most of the observed maxima better :—

TABLE XII.

Epochs of Maxima of T Herculis.

Period.	Assumed Epoch.	Obsd. Epoch.	O – A.	Period.	Assumed Epoch.	Obsd. Epoch.	O – A.	Period.	Assumed Epoch.	Obsd. Epoch.	O – A.
I.	0272	0274	+ 2	LVIII.	9677	9658	– 19	CX.	8257	8259	+ 2
X.	1757	1751	– 6	LIX.	9842	9830	– 12	CXI.	8422	8426	+ 4
XII.	2087	2082	– 5	LXI.	0172	?0158	? – 14	CXII.	8587	8584	– 3
XIV.	2417	2417	0	XCII.	5287	5287	0	CXIII.	8752	?8750	? – 2
XVI.	2747	2743	– 4	XCIV.	5617	5616	– 1	CXIV.	8917	8913	– 4
XVII.	2912	2910	– 2	XCVI.	5947	5944	– 3	CXV.	9082	9078	– 4
XIX.	3242	3247	+ 5	CI.	6772	6771	– 1	CXVI.	9247	9245	– 2
XXI.	3572	3578	+ 6	CIII.	7102	7104	+ 2	CXVII.	9412	9406	– 6
XLIII.	7202	7190	– 12	CIV.	7267	7265	– 2	CXVIII.	9577	9577	0
XLVII.	7862	?7842	? – 20	CV.	7432	7431	– 1	CXIX.	9742	9737	– 5
XLVIII.	8027	8004	– 23	CVI.	7597	?7605	? + 8	CXX.	9907	9906	– 1
L.	8357	8335	– 22	CVII.	7762	7763	+ 1	CXXI.	0072	0070	– 2
LII.	8687	8667	– 20	CVIII.	7927	7929	+ 2	CXXII.	0237	0234	– 3
LVI.	9347	9326	– 21	CIX.	8092	8086	– 6	CXXIII.	0402	0407	+ 5
LVII.	9512	9490	– 22								

The "observed" epochs of Periods XCII. and CXIII. are not calculated like the rest, but are estimated from the appearance of the observations.

It must be admitted that this Table gives no support to the theory of sudden periodical changes of phase which was suggested by Baxendell's observations. It is also clear that the theory of an unchanging period of 165 days suits all the observed epochs except those of Baxendell's later series, viz. Periods XLIII. to LXI.

But the failure in agreement of these epochs is so decided and so consistent that we must suppose either a temporary change in the star's variation or some very strange and persistent error in the observations throughout that whole series. The epochs in the Table from XCII. are those of the B.A.A. observations; but those of Harvard (see p. 492) never differ from them by more than a very few days.

The new B.A.A. observations of R Serpentis have been dealt with in the same manner, and five new epochs (8828, 9188, 9544, 9894, 0248) have been obtained. Compared with the uniform period of 356·7 days suggested in *M.N.*, lxxvi. p. 689, these give residuals of +9, +12, +12, +5, and +2, which are not satisfactory; while the residuals from the "new formula" there proposed are still less so, being −1, −4, −9, −19, and −23.

In the following Table the whole set of epochs is compared with a period of 356·8 days:—

TABLE XIII.

Comparison of Epochs of R Serpentis.

E.	Observed Epoch.	Uniform.	O−U.
31	9582	9556	+26
	240+		
35	0990	0983	+ 7
37	1695	1697	− 2
38	2046	2054	− 8
39	2401	2410	− 9
40	2756	2767	−11
41	3119	3124	− 5
42	3482	3481	+ 1
	241+		
75	5253	5255	− 2
76	5619	5612	+ 7
77	5961	5969	− 8
79	6674	6682	− 8
80	7036	7039	− 3
81	7393	7396	− 3
82	7752	7753	− 1
83	8104	8110	− 6
84	8463	8466	− 3
85	8828	8823	+ 5
86	9188	9180	+ 8
87	9544	9537	+ 7
88	9894	9894	0
	242+		
89	0248	0250	− 2